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PUSA

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Considerable Parts of the WORLD.

VOL. XLIII. For the Years 1744. and 1745.

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M. DCC. XLVI.

T O
JAMES WEST, Esq.
TREASURER and VICE-PRESIDENT
O F T H E
ROYAL SOCIETY
O F
L O N D O N,
F O R

Improving NATURAL KNOWLEDGE,

*This Forty-third VOLUME of
Philosophical Transactions*

Is humbly Dedicated, by

*His Most Obedient, and
Most Humble Servant,*

CROMWELL MORTIMER, R. S. Secr.

Devonshire-Street, near Queen's Square,
L O N D O N.
November 10th, 1746.

ADVERTISEMENT.

Where-ever it is said, at the Head of any Paper, *Here printed with Additions*, or with *Alterations*; It is to be understood, that the Author of such Paper made such *Additions* or *Alterations* himself; for None of them have been made by the Editor. And where it is said, *Presented* on such a Day; It implies that the Paper was not read; the Contents of it being of such a Nature as not to be understood at a bare Reading; and that therefore the Subject in general was only mentioned, or the Title read.

ERRATA.

In Number 471. p. 589. l. 4. omitted, not only a *Pen of Iron*, but also *the Point of a Diamond*,

In Num. 473. in the Contents Art. IV. and p. 47. l. 1. for *Warren*, read *Warwick*.

In Num. 474. in the Contents, Art. IX. and p. 166. l. 19. for *Wintler*, read *Winkler*.

PHILOSOPHICAL TRANSACTIONS.

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and *April, 1744.*

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Printed for C. DAVIS, over-against *Gray's-Inn Gate*
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M.DCC.XLIV.

I. *An easy Method of procuring the Volatile Acid of Sulphur, by Ephraim Rinhold Seehl; addressed in a Letter to the President and Fellows of the ROYAL SOCIETY.*

Gentlemen,

*Read at a Meeting
of the Royal Society,
Jan. 19. 743-4.*

HAVING found that a commodious and cheap Method of obtaining the true volatile Acid of Sulphur was greatly wanting, I take the Liberty of laying such a Method before you, in order to its being disposed of as ye shall think proper.

The Difficulties which attend the making of the *Oleum Sulphuris per Campanam*, even after the Improvements of M. *Hornberg*, are so great, the Process so tedious, and the Produce so moderate, that this Preparation is rarely to be met with in the Shops; but the Oil of Vitriol is commonly sold, and used, in its stead.

Nor, perhaps, if the true *Oleum Sulphuris per Campanam* could be obtained cheap, is this the Thing so much wanted in Chymistry, Pharmacy, and Physic, as the volatile Acid of Sulphur; containing not only the *Oleum Sulphuris*, as it is called, but, at the same time, the pure native *Gas*, or highly volatile Spirit, of the Sulphur; which in our Method is preserved; and which, we have Reason to believe, considerably increases its Virtues, both as a Menstruum, and a Medicine. But of this, Gentlemen, ye are the properest Judges; who are too well acquainted with the Writings and Discoveries of Mr. *Boyle*, *Becker*, *Hornberg*,

berg, and *Stahl*, upon this Subject, to receive any Information from me.

Permit me only to say, it was Dr. *Stahl's* Method of procuring the volatile Acid of Sulphur, that put me upon the Thought and Endeavour of doing the same Thing in a better Manner: And, indeed, I look upon my Method to be no more than an Improvement of his; and this will appear by the Process which follows: Tho' I have Two Ways of obtaining the Spirit of Sulphur; one by the means of fixed Alkali Salt; and the other by means of the same Salt and Quick-lime.

PROCESS I.

To make the volatile Spirit of Sulphur with Alkali Salt per se.

Take a Pound of the Flowers of Brimstone, and Five Pounds of dry fixed Alkali Salt; grind them together, and put the Mixture into an Iron Pot; add, by degrees, a little Water, so as first to dissolve the fixed Alkali; then gradually dispose the Whole to boil, in order the better to dissolve the Sulphur: When these have boiled for a Quarter of an Hour, add more Water by degrees; and, when the Sulphur appears to be dissolved, filtre the Solution; evaporate it to perfect Driness in an Iron Pot, till it almost begins to melt; then take out the dry Powder when cool; put it into a tubulated Retort; which being placed in a Sand-heat, and a Receiver luted on, pour in at the Tube, by degrees, Two Pounds of rectified Oil of Vitriol; and immediately secure the Tube with a Stopple of Chalk, and Luting: Then give a gradual Fire for some Hours, till all the volatile Spirit of Sulphur

is come over; after which let the Fire go out; take off the Receiver, and carefully pour the Liquor into a Glafs Phial, to be stopped with a Glafs Stopper. The volatile Spirit, thus procured, will be about Twelve Ounces in Weight, and appear tolerably limpid, smell extremely quick, pungent, and *Gassy* or sulphureous, almost like the *Gas Sulphuris*, prove strongly acid to the Taste, and in all other Experiments; so that it may be used in the way of a general Acid; being, perhaps, the best, in all respects, that is hitherto known, except the following:

PROCESS II.

To make the volatile Spirit of Sulphur cum Calce.

Take a Pound of the Flowers of Brimstone, Four Pounds and an half of fixed Alkali Salt; grind and mix them well together; put the Powder into an Iron Pot set over the Fire; add a little Water, by degrees, to dissolve the Salt; then boil gently for a Quarter of an Hour; add more Water, and afterwards Three Pounds of strong Quick-lime; let all boil together for a while: When the Solution is complete, filtre the Lixivium, and evaporate to a dry Powder, as in the First Process; put this Powder into a tubulated Retort; and pour on, by degrees, a Pound and an half of rectified Oil of Vitriol; proceed to distil as before: Thus ye will obtain Eight Ounces of a more strong, more acid, and more volatile Spirit, than the former, and of a yellowish Colour.

OBSERVATIONS.

- I. The Proportions of the several Ingredients here set down, I have found, by repeated Trials, to be the best. Five Pounds of Alkali Salt are thus absolutely necessary to dissolve One Pound of Sulphur; tho', when Quick-lime is used, as here specified, Four Pounds and an half of fixed Salt are sufficient; or even Four Pounds, if the Quick-lime be very good and strong: So much doth the Lime strengthen the Lixivium, or enable it to dissolve the Sulphur.
- II. These Two Processes differ somewhat considerably, as to the Quantity and Quality, both of the Spirit and *Caput mortuum* they afford. The Spirit made with Lime is less in Quantity, but specifically heavier, and yet more volatile, than the other: And the *Caput mortuum* with Lime is much whiter, purer, and fitter for making the *Tartarus vitriolatus*, than that made without Lime.
- III. By mixing a Pound of fixed Alkali with the Sulphur at first, boiling them a little, and filtering the Liquor, then adding Two Pounds more of the Salt along with Two Pounds of Lime, I have found, that the Sulphur sooner dissolves, than if I put the whole Quantity in at once; and thus, after the second Filtration, I put in the rest of the Salt and Lime, till all the Sulphur is dissolved; as finding this the readier Way to perfect the Solution.
- IV. In the Distillation, a little of the Sulphur will sometimes sublime into the Neck of the Retort; and this seems owing either to making the Fire

too fierce at the Beginning, or using the Oil of Vitriol too weak: But such a Sublimation of the Sulphur is no farther Detriment to the Operation.

V. When Lime is used in this Process, a considerable Proportion of fixed Alkali Salt may be saved, the Spirit be rendered stronger, and the *Caput mortuum* cleaner and whiter, so as to make an excellent Tartar of Vitriol, by Solution, Filtration, and Crystallization: But it must be observed, That the Produce of this Tartar of Vitriol, when prepared, is not near so large as when no Lime has been used in it; and accordingly I have found, that the dry Powder, remaining after the Solution and Evaporation of the *Sal Alkali* and Sulphur alone, weighs as much as they did originally: Whereas, when Lime has been used, the remaining Powder has weighed half a Pound less than the original Weight of the Sulphur and fixed Salt; which seems a curious Phænomenon; and might lead to farther Discoveries of the Relation betwixt Lime and fixed Alkali Salt, &c.

VI. The Advantages of this Method, in respect of M. *Homborg's*, are,

- (1.) That it gives a much larger Quantity of the Acid of Sulphur. (2.) That it gives a very volatile Acid; whereas his is fixed, so as scarce to differ from Oil of Vitriol. (3.) That it is obtained in a much more easy and cheap Manner. (4.) That this Spirit has, probably, much greater medicinal Virtues. (5.) That it is a much more powerful Menstruum; especially with regard to Metals, and particularly their Crystallization. (6.) That the *Caput mortuum* is a Medicine of great

great Use ; and may defray the Expence of the whole Operation ; being, perhaps, the best Way of making the *Tartarus vitriolatus* perfectly pure and neutral for medicinal Purposes ; its expected Virtues greatly depending upon its being clean and neutral.

VII. Our Method has also several Advantages over Dr. *Stahl's* ; tho' his indeed affords a volatile Acid. But then, (1.) His Method burns the Sulphur, and, consequently, destroys its Texture, and throws off, or exhales, Part of the Spirit or *Gas* ; whereas ours gently dissolves the Sulphur, and only divides it, so as to leave the Acid afterwards separable by a stronger or more ponderous Acid ; and no-way consumes or destroys the inflammable Part, as Burning does. (2.) Our Method is more neat or elegant than his, and affords a larger Produce, at a cheaper Rate, and in greater Perfection, both as a Medicine, and as a Menstruum ; leaving also the *Tartarus vitriolatus* cleaner, and fitter for Use as a Medicine.

VIII. Persons but little versed in chymical Philosophy, and the Operations it makes use of, might be apt to suspect, that this Spirit of ours is not a pure Spirit, or Acid of Sulphur ; but mixed with the Oil of Vitriol, here used as the Medium to separate the Spirit from the Sulphur and fixed Alkali : But ye, Gentlemen, very well know it to be an universal Law, that an heavier or stronger Acid, used in a suitable Proportion, constantly, in these Cases, separates a weaker, and leaves it free to rise by itself in Distillation, as it remarkably does in the present Operation ; where all the
Oil

Oil of Vitriol employed unites with the fixed Alkali, so as to make the true *Tartarus vitriolatus*; and leaves the lighter Spirit quite detached and free to rise, and come over the Helm in Distillation: So that this volatile Spirit and the fixed Oil of Vitriol are by no means the same Thing; nor should the one be used for the other, especially in Physic.

- IX. But tho' the Oil of Vitriol be allowed to differ from the volatile Acid of Sulphur, some may imagine, that there is no Difference betwixt this volatile Acid and the volatile Spirit of Vitriol, as it comes over in the Rectification of Oil of Vitriol; or betwixt our Spirit and the *Gas Sulphuris*, which is extremely pungent and volatile: But whoever attentively examines and compares the volatile Spirit of Vitriol, or the *Gas Sulphuris*, with our Spirit, will soon be convinced of a great Difference; tho', indeed, they agree in the Point of *Gassy* Volatility: For the volatile Spirit of Vitriol is only an impure Phlegm of Vitriol, containing very little Acid, and is chiefly impregnated with the wild Fumes of the Vitriol; so as, upon standing a while, to quit the Liquor, and leave it nauseous, vapid, and gross; whereas the volatile Spirit of Sulphur long preserves its Volatility, the purer *Gas* being here lodged in a pure acid Liquor, less dense and gross than Oil of Vitriol; so that when, by being long unstopped, this acid Spirit loses of its Volatility (as it will do), yet it never loses of its Acidity; and even then appears to be the most pure and perfect Mineral Acid we can any way procure. And, as to the *Gas Sulphuris*, when

when made in Perfection; this is no more than the Fumes of burning Brimstone catch'd and detain'd in Water: So that this Preparation, wanting the Acid, cannot be compared, in that respect, with our Spirit, which has it in Perfection.

X. What the Medicinal Virtues and Uses of our volatile Acid of Sulphur may be, I humbly submit to you, and the learned Physicians, to whom it belongs; only beg Leave to observe, that if what we find in numerous learned Physic-Books be just, there are Hopes, that it may prove a noble Medicine in many Kinds of Fevers, the Small-pox, and even in Plagues. In some of these Books we read, that malignant Fevers are owing to a Superabundance of volatile alkaline Salts in the Body; and, if that be the Case, one might hope to neutralize or destroy such a Superabundancy of volatile alkaline Salts, by the prudent Use of this fine volatile Acid; which is capable of being mixed with Water, Julaps, and most Sorts of Drinks.

XI. I likewise find, That the Origin of all Pestilences and Plagues has been assign'd to the following Causes; *viz.* (1.) The Carcases of Men, Horses, or Cattle, kill'd or slain, and putrefying above-ground by Heat and Moisture, and thus infecting the Air by their noxious, volatile, urinous alkaline Salts, that copiously issue from them in such a putrefying State. (2.) Dead Fish, thrown out of the Sea, and putrefying on the Shore; or Swarms of dead Insects, bred in Fens and Marshes, drowned in the Ocean, and thrown on Shore by the Tides, and left to putrefy in hot moist Climates. (3.) Woollen Goods, Silks, and Apparel,
packed

packed up or worn by infected Persons, or those that attended the Sick, or that came from infected Places. (4.) Unwholsome Diet, or corrupted putrefying Meats, abounding with too subtilized, or too rarefied, volatile, urinous Salts. (5.) Mineral, arsenical, and poisonous Damps, Vapours, Exhalations, &c. arising from Vulcano's, Mines, Grotto's, by means of subterraneous Heats and Fermentations.

XII. It were easy, by natural Reasoning upon these Causes assign'd of the Plague, to shew that Distemper consists in a kind of putrefactive State of the Body, when the Salts are volatilized, unsheath'd, and let loose to tear and wound the Solids, after destroying the Texture; and, consequently, that the volatile Acid, here shewn to be easily procurable, is a natural Remedy in such Cases: But, being sensible how fallible all such kind of Reasoning may be, I intirely submit the Whole to your maturer Judgments; and remain,

Gentlemen,

Your most humble Servant,

Ephraim Rinhold Sechl.

II. *An Observation of a Spina bifida, commonly so termed; by Mr. George Aylett, Surgeon at Windsor.*

September 30. 1740.

Read Feb. 2.
1743-4.

THERE appeared, covering the lower Part of the Loins of a lusty Infant just born, a large incysted Tumour, that seemed capable of containing a Pint of Water, whose Contents had escaped in the Birth from a small Perforation in the Middle of the Cyst; from whence, on Pressure, issued out a bloody Serum.

Flannels, wrung out of an hot, discutient, and restraining Fomentation with Spirits, were twice a Day applied, to prevent its mortifying; to which the upper Part seemed greatly tending.

The first Four Days there appeared no visible Alteration in the Child's Health: She sucked well; was as hearty and strong as most at that Age are; no Paralysis in the Extremities, but a daily Discharge from the Perforation of nigh Two Ounces of the same bloody Serum which at first issued out. The Nurse had observed, that, during all this time, it had not made one Drop of Water.

The Fifth Day the Child was convulsed; which increasing, she died in the Night following.

On the Division of the Cyst, next Day, there appeared a thin membranous Substance, lining it internally; and might be an Expansion of the Membrane which envelops the *Medulla spinalis*. A Number of small Blood-vessels appeared about the Perforation

of the Bone ; and, underneath a small Portion of the *Medulla* of a very thin Consistence. There was no Opportunity of making a further Examination, thro' the Mother's Importunities : But the lumbal *Vertebrae* and *Os sacrum* were taken out, as appears in the Figure annexed.

Tab. I. Fig. 1. by C. M.

A B, The *Vertebrae* of the Loins.

B C, The *Os sacrum*.

CD, The *Ossa Coccygis*.

EF, The *spinal* Processes of the *Vertebrae* of the Loins ; which Spines are here discontinued, and an Opening formed,

FGHI, quite into the Canal of the *Vertebrae* ; so that the *Medulla spinalis* was intirely laid bare without any bony Covering. This Opening has been mistaken for a Parting of the *spinal* Processes into Two Rows ; or as if at *F* they had divided into Two Branches ; the Two Edges *F* and *G* feeling thro' the Integuments like a Bifurcation of the Spine, and so have given Rise to the Notion of a *Spina bifida* ; which Case I doubt whether it ever exists : For a perfect *Spina bifida* must suppose the very Canal and *Medulla spinalis* to divide into Two Branches, the Bodies of the *Vertebrae* to become near twice as wide as usual, and the *spinal* Processes to divaricate into Two Rows or Ridges of Spines. C. M.

Dr. Ratty, late *Secr. R. S.* has communicated a Case like this. See *these Transact.* n. 366. p. 98.

III. *An Improvement on the Practice of Tapping; whereby that Operation, instead of a Relief for Symptoms, becomes an absolute Cure for an Ascites, exemplified in the Case of Jane Roman; and recommended to the Consideration of the Royal Society, by Christopher Warrick, of Truro, Surgeon.*

Read Feb. 16.

1743-4.

AMONG all the Operations of Surgery, perhaps there is not one, of equal Importance, less oblig'd to Improvements, than that of the *Paracentesis*; it being, at this time, notwithstanding the frequent Use thereof, in the same State of Imperfection as when first introduced among us; a mere palliative Remedy, or a Relief for Symptoms. Mr. *Monro* and Mr. *Garengeot* have, indeed, thrown some considerable Lights thereon; the former, * by substituting a Belt, instead of the ordinary long Bandage for compressing the *Abdomen*, and in ascertaining the proper Place of Puncture; the latter, in making the Evacuation at once, and in rinsing the emptied Cavity, to remove the feculent Part of the Waters; which, subsiding therein, and being apt, by its Acrimony, to make rude Impressions on the Viscera, he says, † frequently occasion Mortifications thereof. These are, I believe, the only Improvements therein; at least, that seem to

* *Vide Medical Essays, Vol. I. Art. 18.* † *Traité des Opérations de Chirurgie, Chap. 6. Art. 6. de la Paracentese.*

have sufficient Solidity in them to recommend themselves to our Practice. Its proper Object, an *Ascites*, however, still baffles our Endeavours, and renders the Use of it contemptible and precarious ; always assuring us of a Relapse, by returning as constantly as ever we put this Method alone in Practice to remove it ; so that, even in case of its being consider'd as a palliative Remedy, or a Relief of the Symptoms, by repeating the Use of the *Trochar*, we must suppose, at the same time, an equal Certainty of Pain, continual Anxiety, and, perhaps, Death ; which, at last, must be the inevitable Consequence of it.

From these Considerations, and a sincere Willingness to communicate to others what I think herein has been of Service to myself, I am inclined to believe the following Piece of Practice, considering the Nature and Importance of it, may not be unacceptable to the Public ; not only as it tends to establish an absolute Cure for an *Ascites*, but likewise as it may afford us some Light in the Treatment of an *Hydrocele*, *Hydrops Pectoris*, *Ovarii*, and other Diseases incident to the human Fabric.

In 1742, among a great many Hydropics that fell under my Care that Year, I was called to the Assistance of one *Jane Roman*. She was an Inhabitant of the Parish of *St. Agnès*, near Fifty Years of Age, and confined to her Bed, under that Species of Dropsy called *Ascites*, owing its Rise, some Years before, to the Severity of a lingering intermittent Fever. The most remarkable of her Complaints were, Loss of Appetite, difficult Breathing, unquenchable Thirst, Suppression of Urine, and a short, importunate, asthmatic Cough, joined to that essential
Symptom:

Symptom of the Disease, a large Quantity of extravasated Waters in the Cavity of the *Abdomen*, distending it to an enormous Size, and perceptibly fluctuating. Her more inferior Parts were likewise swoln to an uncommon Magnitude, with livid Spots and Vesications in divers Places. Under these Circumstances, and already fatiated with tedious Courses of ineffectual Medicines, I drew from her (*Sept. 20th*) Thirty-six Pints of a greenish transparent Lymph, by a *Paracentesis* made after the usual Manner; whereby her Complaints vanished, and she was soon re-established on Foot again. With some Part of the extracted Lymph, which I had conveyed to my own House, on my Return thither, I made the following Observations:

OBS. I.

Being as warm as it came from the *Abdomen*, with one Pint of it I mixed the like Quantity of fresh *Bristol* Water; and immediately a slight *Coagulum* ensued.

OBS. II.

In mixing equal Parts of warmed Lymph and *Cobore* Claret together, the same Phænomenon appeared; the *Coagulum* subsided, and the Mixture became milky.

OBS. III.

Being mixed with *Pymont* Water, it manifested little or no Change, only went turbid.

OBS. IV.

I mixed a Decoction of the *Cortex* with the like Quantity of warm Lymph, and it dropped a branny Sediment.

OBS.

OBS. V.

Lymph *per se*, boiled, became gelatinous; but, being mixed with a strong Solution of *Terra foliata Tartari*, it soon resumed its former Fluidity.

OBS. VI.

In bringing the above Mixture to a State of boiling, the Phenomenon of Coagulation appeared more eminently in each of them; especially that with Claret. *Eaton's* Styptic, Tormentil-roots, Pomegranate-peels, and almost every Restrictant, more or less afforded the same Appearances of Coagulation.

Notwithstanding the Disappearance of the Symptoms, and the favourable Prospect that ensued the Evacuation of the Waters, the Relief which she had thereby was only temporary, and of a short Duration: For,

Sept. 30. An Inundation again alarmed her, and obliged her forthwith to remove the Bandage, for Fear of Suffocation. Hence, to the latter End of *October*, she re-filled incredibly; and, notwithstanding any Method used to prevent it, within Forty Days after the *Paracentesis*, there was again collected, in the *Abdomen*, and depending Parts, a Quantity of Lymph, equal to, if not greater than, that which I had but just before thence extracted. All her former Complaints, especially the *Dyspnœa*, likewise returned, and oppressed her more violently than ever.

Oct. 29. The Waters being ready to break their Confines, and the Pain and Distention insupportable under them, she again desired my Assistance to relieve

lieve her. I had, by this time, drawn some Conclusions from the above Observations on Lymph and Restringtons, and flattered myself, that some of them, especially those of the warmest kind, applied immediately to the Parts affected, (the ruptured Lymphatics) must, according to their known *Modus* of Operation, close up their Mouths, and prevent a further Effusion of their Contents, and, consequently, a Return of the Disease.

In order then to obtain this desirable End, I resolved to try their Efficacy, by way of Injection, on the emptied Cavity: And, for this Purpose, the Claret and *Bristol* Water seemed to claim the Superiority in my Esteem; not only as they produced the strongest *Coagulum* with Lymph, but likewise in being the safest, and least liable to create any uneasy Sensations on the *Viscera*. The Experiment, however, being of a pretty singular Nature, I communicated my Sentiments thereof to Dr. *Colwell*, and Dr. *Russell*, Two eminent Physicians of our County, and had the Pleasure of finding them meet in their Approbation: The latter, favouring me with his Presence, saw the Conduct of the whole Affair.

My *Apparatus* was, a large *Trois-quarts*, made on purpose, and dipped in Oil; an *Injector*, capable of containing Two or Three Pints, adapted thereto; and Three or Four Gallons of blood-warm Injection, composed of equal Parts of *Cobore* Claret, and fresh *Bristol* Water; besides Compress, Bandage, &c. as is usual on these Occasions. It was conducted pretty near after the Manner following:

Being seated on her Bed-side, and proper Assistants attending her, I plunged the *Trois-quart* into the
Abdomen.

Abdomen, about Five or Six Inches below, and as much on the Left Side of the *Umbilicus*; and thereby soon discharged upwards of Twenty Pints of such clear briny Lymph as I had before; which Quantity did not exceed Two Thirds of the Whole, though as much as her Strength could well bear: The Claret and *Bristol* Water being then in Readiness, I began to replenish the empty Cavity therewith; but I had scarce injected Ten or Twelve Pints of it, before a *Syncope*, a very material Obstruction, made some Advances, and would fain baffle my Design. Here I perceived the great Expedition necessary in conducting this Experiment; that Symptom being more or less violent, as I happened to be dextrous, or remiss, therein; and was, for the most part, the only one of Consequence that attended it. Quickening therefore my Hand as fast as I was able, and an Assistant stopping the Mouth of the *Cannula* with his Finger, to prevent a Return, I soon brought her up to her former Magnitude, and had the Pleasure thereby of seeing the above Symptom suspended. I had then Time to ask her, what kind of Sensation this new Piece of Practice excited within the Cavity? and whether or not she thought herself capable of undergoing it a second time? She answered me in the Affirmative; and said, It seemed as it were entering her Stomach. Notwithstanding I had Reason to believe my Intentions already answered, as much as in bringing those Restrictants in Contact with the Parts affected, yet, as there was a great Quantity of Lymph left behind in the Cavity undischarged, which, on account of the *Syncope*, I could not well prevent, I imagined their Action, and full Efficacy, might

thereby be, in some degree, interrupted. Every thing therefore being in a favourable Way, I repeated the Mixture for a second Injection (the Claret being in a double Proportion of the Water, to render it the more efficacious for that Purpose); drew off the whole Contents of the *Abdomen* to as much as would flow through the *Cannula*; repeated my Injections as before; and once more, without the least Interruption, replenished her therewith.

This total Discharge, however, made a great Alteration in the Face of Affairs; and her being full, and under Distention, now, altered not, as it did at first, the Case. A pungent Pain possessed her Breast, frequently darting through all the *Viscera*; her Breathing became extremely difficult; her Pulse faltered; the *Syncope* returned; and she became speechless. Under these Circumstances it was high time to conclude my Design; and therefore, having emptied the Cavity, as well as the Violence of them would permit it, I withdrew the *Cannula*, applied proper Compress and Bandage, and finished the Operation after the usual Manner; which was the more agreeable, as it ended with the perfect Recovery of her Senses.

The Day following, (*Oct.* 30.) on repeating my Visit, which I did for divers others successively, I had the Pleasure of finding Things under a favourable Disposition; a gentle *Diaphoresis*, from a liberal Use of Cardiac Medicines, having totally removed the *Syncope*, *Dyspnæa*, and all other Complaints.

November 12. I renewed my Bandage, and set her on Foot again.

The 24th I waited on her again for the same Purpose; found the Swelling in the Extremities going off, her Heart in a chearful Disposition, her Appetite strong, and no Symptom of a Relapse, as she formerly had long before that time approaching.

Dec. 1. the last Visit I made her, she told me, That there was no further Occasion for my Assistance; and that the only Complaint she had then, was, in not having the good Luck to experience this Remedy sooner. Finding every thing therefore in a favourable Way, her Appetite well, her Urine in due Quantity, her Breathing clear, and the extreme Parts of their natural Size, I left her in Pursuit of that Health which she soon acquired, and now enjoys in as eminent a Degree as any other Person whatsoever. To conclude; The Integuments, and other Parts of the *Abdomen*, having not yet recovered their former Tone; or, for aught I know, some of the Injection being coagulated therein; she still is, and ever has been, since the Operation, somewhat above her natural Bigness: However, it is so far from being an Inconvenience to her, that she made me a Visit, on a trifling Occasion, a few Days since, on Foot; and informed me, that she had, of her own Choice, walked Seven Miles that Morning for the Purpose. Her Welfare now is upwards of a Twelvemonth's Standing.

IV. *A Method of conveying Liquors into the Abdomen during the Operation of Tapping; proposed by the Reverend Stephen Hales, D. D. and F. R. S. on Occasion of the preceding Paper; communicated in a Letter to Cromwell Mortimer, M. D. Secr. R. S.*

S I R,

Feb. 22. 1743-4.

Read Feb. 23. 1743-4. **I**T occurred to me, on your reading, *Thursday* last, before the Society, the Case of the Woman at *Truro* in *Cornwall*, who was cured of a Dropsy, by injecting into the *Abdomen* *Bristol* Water and *Cohore* Wine, after having drawn off a good Quantity of the dropscal *Lympha*; that, in case of further Trial, that, or any other Liquor, shall be found effectual to the Purpose, it might be more commodiously injected in the following Manner; *viz.*

By having Two *Trochars* fixed at the same time, one on each Side of the Belly; one of them having a Communication with a Vessel full of the medicinal Liquor by means of a small leathern Pipe: This Liquor might flow into the *Abdomen*, as fast as the dropscal *Lympha* passed off through the other *Trochar*; whereby the dropscal *Lympha* might be conveyed off, to what Degree it shall be thought proper; and that without any Danger of a *Syncope* from Inanition; because the *Abdomen* would, through the whole Operation, continue distended with Liquor, in such a Degree as shall be found proper, by raising or lowering the Vessel with the medicinal Liquor in it.

It

It is probable, that, if the Surface of the medicinal Liquor be about a Foot higher than the *Abdomen*, it may be sufficient for the Purpose.

It were easy to find the Force with which the *Abdomen* is distended by the dropical *Lympha*, by seeing to what Height it arose in a Glass Tube fixed to the *Trochar*; which Tube being taken away, it might, I suppose, be sufficient to have the medicinal Liquor flow in from a lesser perpendicular Height, than that to which the dropical *Lympha* arose in the Glass Tube. I am,

S I R,

Your humble Servant,

Stephen Hales.

V. *An Extract of John Fothergill, M. D. Licentiate of the Royal College of Physicians, London, his Essay upon the Origin of Amber.*

Read March 1.
1743-4.

AFTER all that has been wrote upon the Subject of Amber, its Origin is yet, in a great measure, unknown. Several ingenious Men have searched into this Affair upon the Spot where the Amber is principally gathered: They have related their Observations with great Candour; they have given us the Conclusions they drew from the Facts they discovered; yet without satisfying us intirely about many Particulars.

But,

But, as a Knowledge of the Nature of Things can only be acquired from the Things themselves, I have carefully collected every material Fact I could meet with from those who were best acquainted with the Natural History of this Subject, and whose Industry and Accurateness in observing, and good Faith in relating their Observations, have been generally esteemed unexceptionable. Of these I shall only mention *Wigandus*, *Hartman*, and *Sendelius*; the last who has wrote, as far as I know, professedly upon this Subject.

The Evidence which these Gentlemen afford us, I have endeavoured to throw together, in the most natural Order I could, without respect to any Hypothesis: But as this Enumeration of Facts admits of no Abridgment, my Papers would take up too much room in your Memoirs: Therefore I can only refer to the Essay itself. Upon this Foundation of Facts is built a Discussion of the following Problems:

1. Whether Amber is not strictly a marine Production; or is reduced by some Quality of the Sea-water into the Condition we find it in? Or,
2. Whether it is not to be considered only as a bituminous Body, generated in the Bowels of the Earth? Or, lastly,
3. Whether it is not, in its Origin, a vegetable Production, a Resin; but changed into its present Form by a mineral Acid?

It will only be necessary, in this Place, to mention, that, after having shewn the Difficulty of maintaining the Two first, I have undertaken to support the last of these Opinions.

I endea-

I endeavour to make it appear, that Amber was, in its Origin, a vegetable Resin; the Product, perhaps, of the Fir or Pine Kind; by considering the Appearance of the Substance itself: And that though it has some distinguishing Properties, yet it has many others, which are common to an indurated Resin. Its Aspect, its Texture, its Form, are Arguments for this. The Bodies which it is known to inclose, are urged as Proofs, that this Inclusion could not happen in the Sea, nor in the Earth, but upon its Surface; as the included Objects are mostly Animals, mostly Volatiles too; very few Reptiles, except such as are often found aloft in Trees, as Ants, Spiders, &c. and scarcely ever any Aquatics, are found in Amber. And, I believe, I may challenge all the Cabinets of the Curious to produce one Instance of a marine Body having been found naturally inclosed in Amber. That there are several fictitious ones, is granted.

That this Resin with the Trees which afforded it were buried in the Earth by the Deluge, or by some such violent Renversement, and there constitute the proper Veins of Amber, I likewise endeavour to make appear, from the same Evidence of Facts. The Substance of which these Veins consist, hath several genuine Characteristics of Wood still remaining. The Texture of this Substance is often an undoubted Proof of what it hath been; being fibrous, and, when dried, swims in Water, and burns like other Wood. The Amber is not disposed in these Veins in one continued *Stratum*; but Lumps of it are irregularly disseminated through the Whole of what I call the woody Mass.

A Difficulty, which naturally offers itself in this Place, is attempted to be removed :--- What Proof have we, that this, which is called Wood, is not mere fossil Wood, the Product of Creating Power, exerted in the Place where it is now found? --- It is answered, That as there are undoubted Proofs, that many Substances now occur, where they were not originally framed, we are under no greater Difficulty in accounting for the Change of Place in one than the other. It is known, that the *Exuviae* of Fishes are sometimes found on the Tops of the highest Mountains. The Bones of large Animals are met with at prodigious Depths, where Nature never formed, nor Art conveyed them. Whole Woods are found underground. The Cause that effected these, was capable of the other.

Yet, allowing these Allegations to be just, by what Causes is this Change produced? It is urged, That Time is one of the Causes; and that the rest is completed by the Acid of the Earth, a vitriolic mineral Acid. It is proved, from the Facts above-mentioned, That such an Acid is present where-ever Amber occurs in its *proper Matrix*: That it is sometimes found in the Amber itself, in its genuine Appearance; That the Acid of the Salt of Amber appears, from Experiments, to be vitriolic; That common Turpentine (a known vegetable Resin) affords, by proper Management with a vitriolic Acid, a considerable Portion of the same chemical Principles that Amber does; That those Pieces of Amber, which have been found soft and imperfect, are nearly related to a vegetable Resin: In short, it is endeavoured to be proved, That we have the Ingredients of
Amber

Amber in our Power, and that nothing is wanting but a successful Application of them to each other; at least to procure the medicinal Preparations of Amber at an easy Expence. Time and repeated Trials may, perhaps, ripen this Beginning, in somebody's Hands, into an happy useful Imitation of this valuable Substance.

This Account is concluded with an Inquiry into the medical Virtues of Amber, and some of its principal Preparations. It is observed, that a Substance of so firm a Texture, as scarce to yield to any common *Menstruum*, is not likely to produce any considerable Effects upon the human Body; and that, indeed, there are very few genuine Instances recorded of any: That busy Imagination might, probably, at first, introduce it, Prejudice support it, and engage Men of Parts and Authority to recommend it to their inattentive Successors.

I shall finish this Abstract with remarking, That, were some of the leisure Moments of Men of great Abilities and Experience devoted to inform the World of the Inefficacy of such Methods and Medicines as they have proved to be so, Physic would be reduced into narrower Bounds; they would merit the Thanks of every one in the Profession; and Posterity, at least, would commend their Endeavours.

VI. *Remarks on Stones of a regular Figure found near Bagneres in Gascony: With other Observations, communicated by Monsieur Secondat de Montesquieu, of the Academy of Sciences of Bordeaux, in a Letter to Martin Folkes, Esq; Pr. R. S.*

Read Mar. 8. 1743-4. **T**HOUGH the Spring called *La Fontaine du Salut* is at a good Distance from the Town of *Bagneres*, it is, nevertheless, as much frequented as any in that Country; and, besides its admirable Effects in curing a great Number of Distempers, it likewise offers, to the Eyes of the Lovers of Natural History, a very remarkable Singularity.

In the first Bath, through which the largest of the Two Branches of the Spring flows, there are found, from time to time, small Stones, of the Colour of Iron-rust, and of a regular Figure; being either Parallelopipedes with oblique Angles, of which the Sides are unequal; or small solid Bodies with Six Sides, only differing from Cubes or Dice in this, that the Surfaces are not perfectly perpendicular one to another, but a little inclined; as also commonly longer than they are broad, and broader than they are high.

The largest which I have seen were but 11 Lines in Length, $9\frac{1}{2}$ in Breadth, and 6 in Height: They are mostly a great deal smaller. I have one which is very odd, being a Parcel of an hundred in one Lump.

Lump. There are some on which one may observe shining *Striæ*, that seem to be of a metallic Substance.

I have heard, that a great many Stones like these are also found on the Sides of a Brook in *Spain*; from whence, without Doubt, they got the Name they are commonly called by, of *Ferreles d'Espagne*.

About Two Months ago, happening to take a Walk in the Road newly made between *Bagnères* and the *Fontaine de Salut*, I perceived, that, in digging the Ditch on the Side of the Road, the Workmen had laid open a Rock of a Sort of imperfect Slate, but softer, and of a lighter Colour, than Slate commonly is. The Rock itself is composed of Layers or Beds lying almost parallel one over the other: The Substance of the Slate seems to be a Composition of Fibres or Strings, placed on the Sides of each other, and equally inclined to their Beds or Layers; whence it comes, that, upon breaking them with a Hammer, the Pieces, sometimes, are pretty like the Figure of a regular Parallelopiped with oblique Angles.

Upon a narrower Examination of this Sort of Slate, I found a great Number of Parallelopiped Stones, like those before spoken of, only smaller: I have seen them of all Sizes, from those in which the largest Side is but of Two or Three Lines. I observed also, after having broken to pieces several little Bits of Slate, certain black Spots; which, by the Help of a Microscope, I found to be real figured Stones.

Besides this, I took notice, that every one of these Stones, as long as it remains in the Rock, is always found between Two Bundles or Clusters of transparent Fibres, of which, generally, one is placed on

the one, and the other on the opposite Side. These Bundles are larger in great Stones: Those which seem, to the naked Eye, to be but small black Spots, are, nevertheless, accompanied by their Bundles.

I have some of these Stones, where the transparent Fibres, of which every Bundle is composed, had left a Vacancy in the Middle of their *Axes*: This sort of Conduit being coloured with a Matter of a rusty Colour, one finds likewise, sometimes, between the Fibres a little of this rusty-coloured Matter; and now-and-then meralllic and shining Veins. One might say, that the Use of these transparent Fibres is the same as of Strainers; which (let the Matter be of a metallic Nature or not) suffer nothing to pass thro' them to the little Stones, but such Particles as are proper to advance their Growth, and so to serve them as it were for Roots. In Bits of imperfect Slate, tho' harder, and of a bluer Colour, than the Sort I before mentioned, are found small Stones, of a like Figure, but different in this, that they are of a fine and shining brass Colour. They are, as well as the others, accompanied by transparent Lumps.

One meets likewise with large Stones of the Colour of iron Rust in several Rocks thereabout. It is probable, that the *Fontaine de Salut* passes thro' one like that I have described; and, if it meets with any of these figured Stones in the Sides of the Conduits thro' which it passes, it easily loosens them, and carries them along with it. The Bundles of transparent Fibres stick pretty fast to the Slate or Rock, but are slightly fastened to the little Stone, from which they are very easily separated. Hence it comes, that all those which fall into the Bath, or
Fontaine

Fontaine de Salut, are got thither without their Bundles.

The Formation of these Stones deserves to be examined : If it was carefully studied, it might, perhaps, give Light to the Formation of other figured Stones. It even seems, that, by beginning with this Figure, which is the most simple that one can imagine, one may the rather hope for Success in the like Inquiry.

This Parallelopiped Figure with oblique Angles is common to many Stones in the Country of *Bagneres*, and the neighbouring Mountains. Several Crystallizations of the Grotto of *Campan* break into Fragments of this Figure : Those which hang down from the Top of the Vault of that Grotto, are, originally, small hollow Pipes, formed by the Water which trickles down Drop by Drop ; and whose outer Surface, fixing themselves by their small Bases, forms, by degrees, a sort of blunted Pyramids, which, like so many Rays from the Axis, which is nothing but the hollow Pipe, become solid at last. This Axis seems composed of Plates, almost cylindrical, laid one over another ; but, if broken, the Whole divides into Fragments of a Parallelopiped Figure. The blunted Pyramids, that are about the Axis, divide themselves at first into other blunted Pyramids ; but, afterwards, almost all these Fragments divide of themselves into other Fragments of a Parallelopiped Figure.

The Stone of the Mountain of *Barege*, upon which the *Asbestos* grows, breaks also constantly into Fragments of the same Figure.

I have likewise preserved a Bit of Rock half transparent, the Fragments of which are like the others.

Having

Having seen several Productions of Nature, in which one discovers, that the Figure I have been speaking of so remarkably prevails, I was nevertheless surprised; when I found the same Figure in the Sediment of the Water of the *Fontaine de Salut*.

I had let a considerable Quantity of the Water of the mineral Spring evaporate; there remained a shining Dust, in which I could distinguish nothing. I then looked at it through a Microscope; and, among several Crystals of a less regular Figure, I found many which were quite regular and well-shaped, with Six Faces, and oblique Angles. Several Persons, who have, at different times, been Eye-witnesses of this Sight, have been well satisfied with it.

The Waters of this Spring contain no Iron, as it is commonly believed. When you put the Tincture of Galls in it, it grows neither black nor red: This Mixture only turns it a little, and makes it look whitish, after having stood some Hours.

When these Waters are evaporated by a mild and equal Heat, the small Crystals are found swimming on the Surface; where they join, and form a Film upon the Water; some of which sticks also to the Sides and the Bottoms of the Vessel. Those Crystals which are formed first, are insipid; but those which are produced towards the End of the Evaporation, are, indeed, of the same Shape, but of a tart and saltish Taste. There remains yet a little of this Matter, which cannot be reduced to very regular Crystals: It is of a very sharp and pungent Savour, but has nothing of the prevailing Character of Acid or Alkali; at least, it makes no sensible Impression upon blue Paper.

The Waters of the Spring *du Pied* have the same Quality as those of the *Salut*: They produce the same Effect when mixed with Galls; yield Crystals exactly of the same Figure, but in yet greater Quantity.

I have not made the same Experiments with the other Springs at *Bagneres*; but, it is probable, they do not differ from the former, except as to More or Less.

To conclude these Observations; I have also found, at *Bagneres*, a particular aquatic Plant, which I had seen, for the first time, in the great Bason of the boiling Spring at *Dax*: It bears neither Fruit nor Flower, as far as appears; its Substance is intirely composed of small Bladders full of Air; the Surface of it is like Net-work or Canvas; it grows only in the hottest mineral Springs; it may be found at the Spring, called, *de la Reine*, at the *Bath des Pauvres*, and at the *New Spring*; but most plentifully at that Place where Part of the *Spring de la Reine* issues out of a Rock near the *Capuchins*. Nobody, as far as I know, has ever spoken of this Plant, before I gave an Account of it Two Years ago, at the public Resumption of our Academical Meetings. The Vegetation, and particular Qualities of it, may, perhaps, deserve to be more narrowly examined; and I believe it may be properly called, *Fucus thermalis vesicularis*, *Superficie reticulari*.

However different the preceding Subject of my Discourse may be from what I am next going to speak of; I beg Leave to set down One Observation more, which I have made on the *Pie du Midy*, and of which the Result may prove curious.

It is well known, that the greatest Degree of Heat in common Water is that which it acquires by boiling; that is to say, if Water is put upon the Fire, it grows by degrees hotter and hotter, till it quite boils; but, after that, though there be never so much Fire added, and it stand never so long upon it, it will never grow hotter than it was on the first Instant, when it began to boil. Hence the Degree of Heat of boiling Water is looked upon as fixed and invariable.

Fahrenheit, that ingenious Master in Mechanics, so well known by his Mercurial Thermometers, is the first who has remarked the contrary. He observed, that the Heat of boiling Water was greater when the Air was heavy (that is to say, when the Mercury stood higher in the Barometer); and, on the contrary, the Heat was less when the Air was lighter.

Mr. *Le Monnier* the younger, who has obliged us with a Translation of Mr. *Coté's* Lectures in Natural Philosophy, with excellent Notes upon the said Work, has put *Fahrenheit's* Discovery past all Doubt, and has very much improved it.

On the 6th of *October* 1739, being provided with a Barometer, and a Mercurial Thermometer of M. *Delisle*, he climbed up to the highest Top of the *Canigou*, a Mountain in *Roussillon*, which passes for the highest among the *Pyrenees*: There he found his Barometer to stand at 20 Inches $2\frac{1}{2}$ Lines; whilst at *Perpignan* it stood at 28 Inches 2 Lines. The Difference between the Heat of the Water which he boiled there, and that which he boiled at *Perpignan*, was 15 Degrees of his Thermometer.

The same Thermometer being surrounded with Snow, the Mercury fell down to the same Degree as pounded Ice had made it do at *Paris*.

Hence he concludes, that the Heaviness of the Air has a sensible Influence on boiling Water; but that it in no way alters the Term of Congelation.

All these Particulars may be seen, *p.* 408. of *Coté's* Experimental Lectures; and in the Memoirs of the Academy of Sciences of *Paris*, *Anno* 1740.

This is the same Experiment which I have repeated on the Top of the *Pic du Midy*; thinking that so singular a Fact ought to be observed more than once.

I carried Two Barometers, the Tubes of which the Reverend Father *Francis* had been so good as to fill for me with great Care. I had likewise with me Two Mercurial Thermometers, upon which I set the Degrees at *Bagnères*: I took the fixed Terms of the Graduation; that is to say, that of Congelation, and that of boiling Water, afterwards putting nought to the Term of Congelation. I marked 180° Difference between this Term and that of boiling Water.

Being come to the highest Top of the *Pic du Midy* on the 9th of last *July*, the Mercury rose in one of my Barometers to 20 Inches 2 Lines; and in the other, to 20 Inches $1\frac{1}{2}$ Line. I surrounded my Thermometer with Snow, and the Mercury fell exactly to the same Degree as the Snow had made it fall to at *Bagnères*. Afterwards I plunged it into boiling Water; whereupon the Mercury rose to 165° of my Graduation: So that the Difference between the Heat of boiling Water on *Pic du Midy*, and that at *Bagnères*, consisted of 15°.

At my Return to *Bordeaux*, I observed, that I had marked the Term of boiling Water at *Bagneres* less high by $3\frac{1}{2}$, than at the Term of boiling Water at *Bordeaux*, taken at the time when the Barometer was at 28 Inches 2 or 3 Lines: Therefore having anew graduated my Thermometer the 165th, the Degree of the former Graduation fell now upon the 162^d; so that the complete Difference between the Term of boiling Water on the Top of the *Pic du Midy*, and that of the same at *Bordeaux*, the Barometer being at 20 Inches 3 Lines, amounts to 18 Degrees on the Thermometer of *Fahrenheit*.

Now the Conformity between the Observation made by M. *Le Monnier*, and this Repetition of the same Observation, can hardly be greater; seeing the Heights of the Barometers are almost the same; and the 15 Degrees of Difference, found by M. *Le Monnier* on *De Lisle's* Thermometer, amount precisely to 18 Degrees on the Thermometer of *Fahrenheit*, which I made use of.

The Eye-Sucker as seen by the Microscope

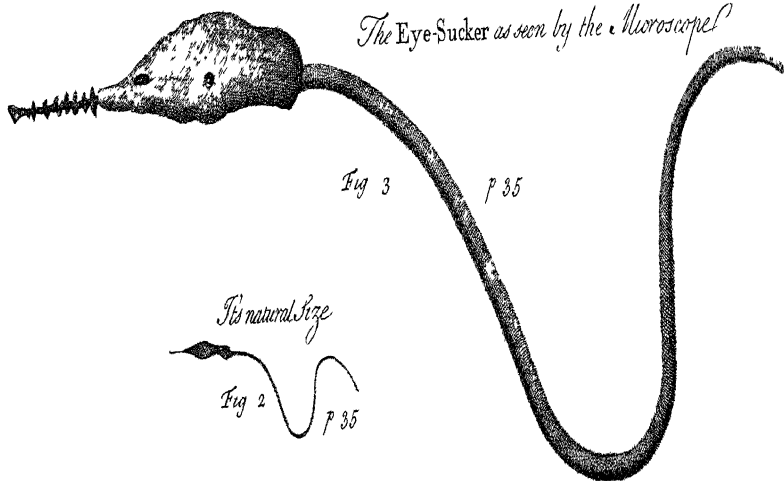


Fig 3

p 35

Its natural size

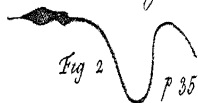


Fig 2

p 35

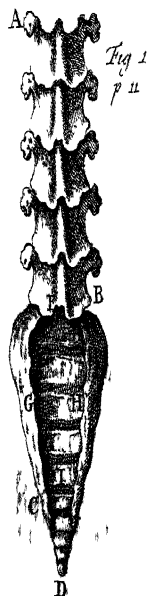


Fig 1

p 11

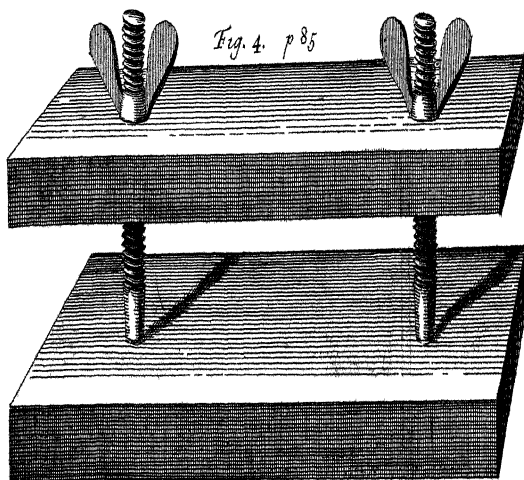


Fig. 4. p 85

VII. *A Letter from Mr. Henry Baker, F. R. S. to the President, concerning a new discovered Sea-Insect, which he calls the Eye-sucker.*

S I R,

Read March 8. 1743-4. I WAS lately favoured with a Couple of small Sea-Insects, by a Gentleman, who told me, they were found fixed by the Snout to the Eyes of Sprats; that they are often observed sticking there, and may consequently be supposed to suck their Nourishment from thence.

As I do not remember this Insect has been yet mentioned by any body, I hope I shall be excused for laying before you a short Description of it, with a Drawing thereof, as seen by the naked Eye, *TAB. I. Fig. 2.* and another as magnified by the Microscope, *Fig. 3.* I shall likewise beg Leave to distinguish it by the Name of *Eye-sucker*, as that Name conveys an Idea of the Manner how it lives.

The Length of this little Creature from End to End is near Three Inches, whereof the Head is about one Quarter-part. Its Body is somewhat thicker than an Hog's Bristle, and of a pleasant green Colour. A Gut seems running through it, and terminates at the *Anus*. The Head is light-brown, twice the Thickness of the Body, and of an oblong Figure, tapering towards the Snout. It has a Pair of fine small black Eyes, and a Couple of Holes, at some Distance forwards, which probably are its Nostrils.

But the most remarkable Part of the Head is its *Proboscis* or Snout; which is nearly half its Length, and does not end in a Point, but spreads at its Extremity with a considerable Aperture. This Snout appears of an horny Substance, and has, on every Side, several large Knobs or Protuberances; whereby, when once insinuated into the Fish's Eye, it must necessarily be fixed there, so as not easily to be removed. But this the Figure will more expressively demonstrate.

As the Insects, whence I take this Description, were dead and dry, it cannot possibly be so exact and particular; and is more liable to Mistake, than if they had been living. But I hope it may serve to give some Idea of this odd Creature, till a better can be obtained from some curious Observer, who may get a Sight of it alive. I am, with the utmost Respect,

S I R,

Your most obedient humble Servant,

March 8. 1743-4.

H. Baker..

VIII. *Some Observations on the Hardness of Shells, and on the Food of the Soal-Fish ; by Mr. Peter Collinson, F. R. S.*

Read March 15. 1744. **I** HAVE observed some Circumstances in the Structure of Shells, which I know not whether they have been sufficiently attended to ; I mean their durable Hardness and Polish ; which, in the slenderest of these Bodies, is very remarkable ; and it is unknown how long they will resist the Injuries of Time and Weather.

I saw an Instance of these permanent Qualities at *St. Edmond's-bury*, in the Ruins of that Abbey, which is built of a kind of Stone composed of Grit or Sand, interspersed with an infinite Number of very minute Shells, which appeared to be a Species of smooth shining Cockle. In several Parts of the Building, much exposed to the Air, the Sand was mouldered away ; but these exceeding small Shells remained intire, and their Polish not in the least decayed.

It may be alledged, that a petrifying Juice, the same that had united the Particles of the Sand together, had likewise hardened these, and rendered them more durable than Nature had formed them.

But, as we have many Instances of Shells retaining their natural Politure and Firmness, where no such Allegation can justly be made (for Instance, the Shells found in Chalk-pits, in Loams, and several other Places, where no such Juices are hitherto proved

to

to exist); yet of so tender Shells, long exposed to the Weather, and still remaining uncorrupted, the Instance mentioned above is the most singular I know, be the Cause of their Duration what it may.

The other Observation, which I would offer to your Consideration, relates, in part, to the same Subject, but of a different Nature. I was led into it by the following Circumstance:

In *September* last, having bought some Soal-fish, before they were skinn'd, I observed their Bellies were prominent and hard, as if they were full of large Rows; but, instead of that, their Guts were filled with Shell-fish, a Species of *Pectunculae*.

Before these Shells were taken out of the transparent Guts of the Fish, the Whole had very much the Appearance of Strings of Beads, or Necklaces; the Interstices betwixt the Shells occasioning this Resemblance. Upon taking the Shells out, I observed, that some of them were almost intirely dissolved, others partly so, but many were whole and intire.

It is well known in Natural History, that Shell-fish are the Food of several Species of Fish. The Sea-Porcupine, and a kind of Ray, are known to subsist chiefly upon them; but then they are wonderfully provided with a suitable *Apparatus* for reducing them into a State more fit for Digestion: Their upper and under Jaws are hard enough to break or grind almost the strongest Shells to the Condition of Pulp.

But the Soal-fish has nothing of this kind: She feeds, we see, on Shell-fish, but digests them not by Attrition; for neither her Mouth, nor any of her

Viscera, are framed for this Purpose ; but, as it would appear from the preceding Account, by a proper *Menstruum*, which is prepared in the Body of this Animal.

Thus we see, *Shells*, which can resist the Teeth of Time, the Inclemency of Seasons, and lie, without apparent Decay, for unknown Ages, in the Bowels of the Earth, reduced, in all Probability, in a little while, almost into a State of Fluidity, by the Juices of a small tender Animal.

Is it the Juices of the whole Animal, or is it the Consequence of such a kind of Nourishment, that renders this Fish so delicious?

It would seem no unnecessary Disquisition of those who are properly qualified, to search, with some Care, into the Nature of this Solvent. It is probable, that either the Juice itself, or the Fish under proper Management, may be rendered more beneficial than has hitherto been expected.

IX. *Historia brevis Morbi, quo decessit Reverendissimus Pater Jos. Bolognini, Abbas SS. Bonifacii & Alexii de Urbe, atque Congregationis Hieronymianæ Procurator generalis; excerpta per C. M. R. S. Secr. ex Epistola ab Domino Johanne Francisco de Camillis, M. D. scripta ad Virum clarissimum Didacum de Revillas, Abbatem Hieronymianum, in Romano Sapientiæ Archigymnasio publico Matheſeos Professore, Regalis Londini Societatis Sodallem, &c.*

Reverendissimus pater, 50 major, viribus tamen florens, staturæ proceræ, habitus obesi, & admodum sanguinei, liberaliter sibi victum indulgens, *Mediolani* (ubi natus est), *Bononiæ*, & *Romæ*, degens, frequenter, mensibus præsertim *Augusti* & *Septembris*, correptus est febris tertiana, modo simplici, modo duplici, vomitu, vomendique nisibus, plerumque conjuncta. Hanc vero, alvo soluta sectaque vena, *Peruvianus* cortex sollicitè adhibitus feliciter semper extinxit. Vomendi incitamentis sæpius obnoxius fuit.

Proxime superioribus annis non infrequenter aliquot a pastu horis questus est de molesto quodam stomachi ardore, ad quem levandum, vespere & mane ante & post *Chocolatæ* potum, aquam quandoque frigidam, vel, pro tempestate, calidam, bibere consuevit. Quotannis primo vere, ventrem mitiori aliquo medicamine

camine relaxare, & venam secare, ad memoratas autumnales arcendas febres in more posuit. Vere nuper elapso, assidua & graviora passus est animi pathemata, intensiores subiit labores: fuit ideo tristis, infomnis, sed inprimis cogitabundus. *Julio* mense, miliari rubra, & prurienti purpura, sed absque febre, aspersus est; ad quam curandam, lactescentes seminum peponum expressiones, & aquam, modo simplicem, modo acredine citri suaviter alteratam, copiose potavit. Hujus mensis 26. vigenti adhuc eadem purpura deambulatione calefactus, quiete postea, & aëris afflatu, admodum refrigeratus est. Itinere iterum incaluit, & mora prope fontem habita a frigidiusculis Etesiis, [N. W.] graviter offensus est. Vespere parce coenavit; præviis horroribus & rigoribus, febre correptus est, vomitione, evomendique conatibus insuper adjunctis: mox insequente febrili calore leni, minime molesto, nocte placide quievit. Crastino die melius se habuit; insequenti vero die rediit febris cum horrore, vomitione, & gravi capitis dolore.

Jamprimum accitus Dominus *Camillus* invenit R. P. febris afflictum nunc inclinante, memorata purpura non amplius apparente. Nocte erupit sudor, & febris dimissa est. Potionem laxativam, ex manna & cremore tartari, & acredine citri, ad mentem clarissimi *Frid: Hoffmanni*, haud insuaviter paratam, crastino assumsit mane. Alvo moderate soluta, melius se habuit. Excreta magna ex parte fuerunt biliosa, quæ sparsim colore quodam *Chocolatam* æmulante inficiebantur. Hora 18^a redit febris, cum consueta vomitione, vespere vero inclinans. Nocte sumpsit *Cort. Peruv.* scrupulos duos, in bolum compactos; sed recrudit febris. Vigil & inquietus æger corticem

F

evomuit,

evomuit, viscidæ tenacique, atque eodem *Chocolatam* amulante colore nonnihil perfusæ lymphæ permistum; cujus quidem coloris materiam, non nisi in pulverem quodammodo redactum sanguinem fuisse, ingravescent morbus magis magisque declaravit. Postridie pulsus facti sunt parvi & frequentes: accessit gravis & molestus sinistri orificii ventriculi dolor, & quædam in sinistrum latus decumbendi difficultas. Manus cæteroquin tactum non incommode inibi patiebatur; utrumque pariter hypochondrium molle erat & tractabile: hodie iterum bolum e *cortice Peruviano* deglutivit; paulo post violentiores passus est vomendi impetus, haustibus aquæ subtepidæ sedati. Hora 19^a, *corticis Peruviani* pulv. scrupulos duos ex haustulo aquæ cardui benedicti potavit: spongiis aqua imbutis hypochondria fovebantur; & oleum amygdalinum sine igne extractum subinde hausit. Post meridiem febris remisit; sumsit æger *corticis Peruviani* drachmam unam ex aqua cardui benedicti; post 4 horas item. Cras mane excreta rubente floridoque deprehendi aspersa sanguine: permanebant adhuc ventriculi passio, vomendinis, inquietudo, & non parvus virium languor. Paullum ante meridiem insensibiliter recurrit febris, pulsus evaserunt debiliores & frequentiores. Adaucta videbatur voluntas vomendi; & per vomitum ejecta est viscidula lympa sanguine conspersa; successive superne per os, & inferne per alvum, atrum & coagulum sanguinem copiose ejecit æger. Confectionem *Hyacinthinam*, in aqua totius citri solutam, parce & per intervalla sorbuit. Post meridiem, senior medicus accersitus est in concilium; unde tria decreta sunt; sanguinis missio; febrifugi corticis reiterata potio, permixti bolo *Armenæ*, vel terræ sigillatæ,

ac

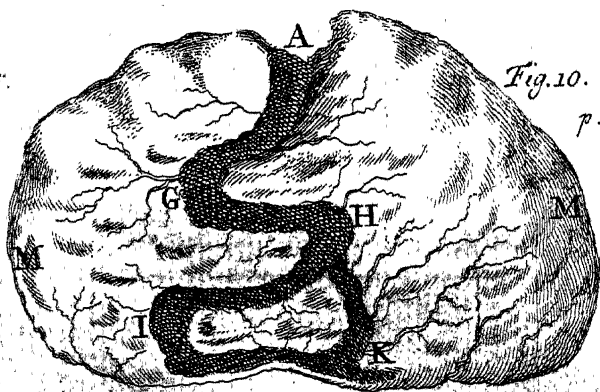
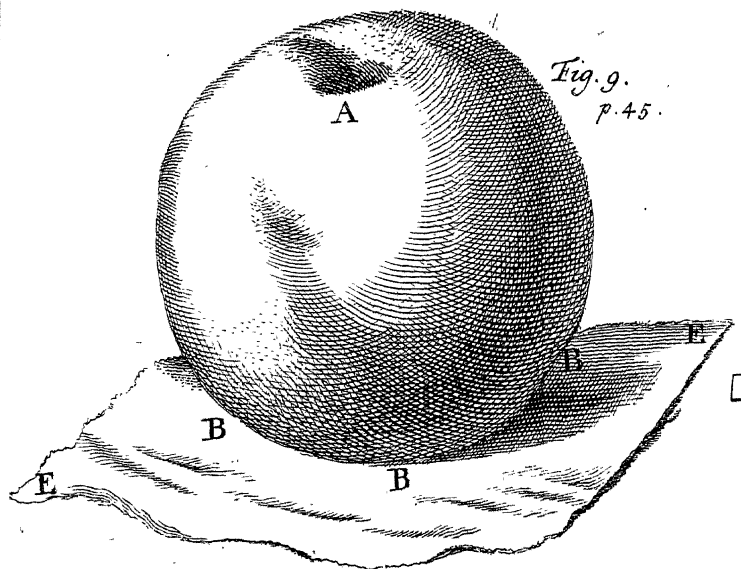
ac in aqua plantaginis soluti; & selectus & moderatus usus cordialium; a V. S. æger nil profecit; immo debilior evasit. Jusculum exinde, ut reficerentur vires, exhausit ex carne vitulina, ovi vitello superaddito. Haud multo post brevissime deliravit. Intra horas tres quatuorve obmutuit; & tertia exinde hora tandem, non sine summo angore, una & anhelitu, frigido caput perfusus sudore, piam animam Deo reddidit, sexta ab invasione febris, quarta a decubitu labente die. Re-
jecti sanguinis pondus æquabat libras fere octo; *Peruviani corticis* deglutiti uncix dimidium; & olei amygdalini uncias quatuor.

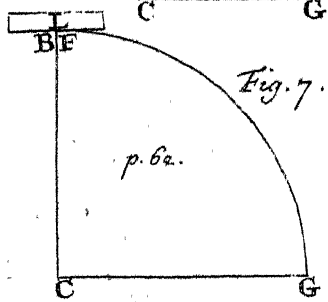
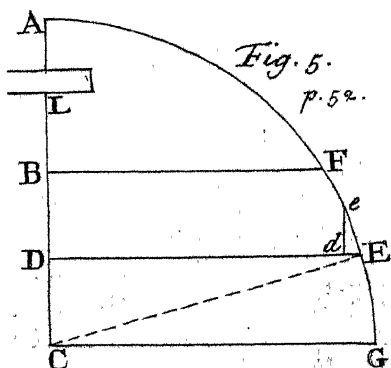
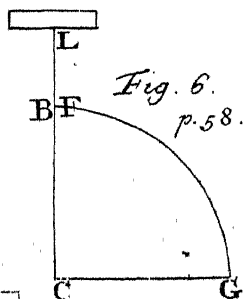
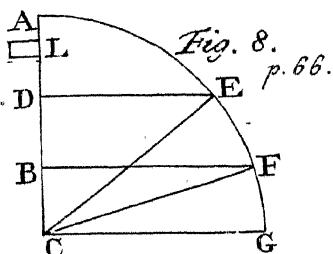
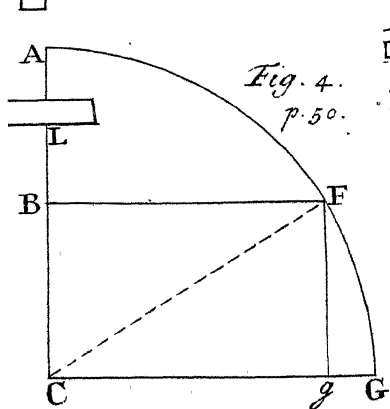
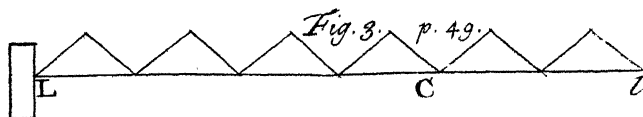
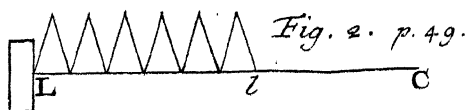
Transactis horis decem & octo aperiendi cadaver mihi munus assumpsi; & occurrit primo corpus solidum, & succi plenum, abdomen insigniter elevatum, & dorsum subnigris quibusdam maculis variegatum.

Universalibus sectis, diductisque tegumentis per amotum sternum in thoracem viam referavimus. Exemimus spiritualia viscera, & exsectis sanguiferis modicum fluxisse sanguinem deprehendimus. Exterior pulmonum facies in parte præsertim postica nigricans apparuit. Pertractatis deinde pulmonibus, in præternaturalem tensionem, & duritiem, eosdem abuisse; & diversimode dissectis interiorem eorundem substantiam sanguine fere orbatam offendimus, ac colore *cophæ* proxime æmulantem. In pericardio reperta est modica lymphæ, eaque admodum obscura. Cor invenimus debili atque flaccida substantia donatum. Patebat in eodem foramen ovale; illudque adstantibus spectandum exhibuimus. Auricularum, & ventriculorum cavitates modicam sanguinis nigri & coagulati asservabant quantitatem. Diaphragmatis convexa pars ventriculo respondens inflammata;

concava vero eundem tangens, nigricans offensa est. Inibi etiam sanguifera vascula turgebant, & elevabantur. In aliis thoracis partibus nulla labes emicuit.

Sectione producta abdomen aperuimus, quod multo adipe refertum erat. Omentum vidimus crassiusculum, naturaliter tamen constitutum. Hepar apparuit externe in parte convexa colore prope naturali; in concava vero, qua ventriculum tangit, obscuro & atro. Internam ejusdem substantiam subpallidam & debilem, cultro haud difficulter cedentem, & sanguine propemodum orbatam adinvenimus. Lienis substantiam conspeximus nigricantem, & plane corruptam; quæ ab investiente membrana, tanquam a marsupio continebatur. Renes, mesenterium, & reliquæ partes, ferme carebant sanguine. In majorem nos traxit admirationem canalium alimentorum perlustratio. Tota enim œsophagi interna facies, inequaliter tamen, nigro & coagulato aspergebatur cruore. Ventriculus tumens & inflammatus visus est. Ejusdem cavitas magnum aquei fluidi, sanguini atro & foetenti commixti claudebat pondus. In eodem fluido quasdam olei amygdalini supernatantes guttulas deprehendimus. Vasa ventriculum perreptantia sanguine erant turgida, & elevata, ea potissimum parte, qua breviora, sic dicta, locantur. In fundo ad distantiam quatuor ferme digitorum a pyloro præter expectationem conspicuus factus est tumor, orbicularem figuram præseferens (TAB. II. *Fig. 9.*) ad instar mali aurantii, & uncias quinque fere ponderans. Elevabatur hic inter ipsius ventriculi membranas. In eminentiori ejusdem parte foramen conspeximus pariter orbiculatum, quod dimidii um digiti transversii latitudinem adimplebat. Tumor præfatus externe ex atro rubescebat. Quoad substantiam
sarcomatosus.





sarcomatosus, & de genere carcinomatum, fuit habitus. Tota insuper intestinalis longitudo externe rubebat; immo intestina tenuia evirescebant. A pyloro per totam buodeni extensionem insignis sanguinis atri & coagulati quantitas est adinventata. Brevis jejuni pars nullo foedebatur cruore, & reliquum ejusdem cum adnexo ileo, partim atro & coagulato, partim florido & rubente, repletum erat sanguine. Colon per totum, & præsertim ejusdem amplior pars, uti & intestinum rectum, putrido & foetido cruore onerata, detecta fuere, quin ulla in toto intestinorum systemate appareret sanguiferorum dehiscencia.

Sequuntur delineationes memorati tumoris, cujus dissectionem non paucis ab extispicio peracto remoravimus horis.

Vide TAB. II. Fig. 9 & 10.

Fig. 9. Exhibet naturalem tumoris magnitudinem & statim a cadaveris sectione figuram; quæ post 18 horas paullo compressior fuit, ut in *Fig. 10.*

A, Foramen in eminentiori tumoris parte, ejusdemque medietate adaperitum, orbicularem figuram præ se ferens.

B B B, Inferior tumoris pars, sive basis, ventriculi fundo ad distantiam 4 dig. transversorum a pyloro firmiter innixa; ubi vasculis sanguiferis refertur in ejusdem corpus extensis.

E E, Ventriculi pars secta & diducta, ut expositi tumoris magnitudo & figura commodius conspicerentur: production interioris ventriculi tunica tumorem investiente.

Fig.

Fig. 10. Demonstrat verticalem tumoris sectionem.

A, G, H, Flexuosa tumoris cavitas, ex foramine *A,*

Fig. 9. oblique in ejusdem corpus descendens, quæ in centro in duas minores cavitates *H I, H K,* dirimitur.

I K, Alia major in fundo tumoris cavitas, per quam ad præfatas minores patet aditus. In hoc tumoris cavo modicum offendimus ichoris, qui solo contactu argenteum perspicillum denigravit.

M M, Interna & *sarcomatosa* tumoris substantia.

X. *A Letter from James Jurin, M. D. F. R. S. & Coll. Med. Lond. to Martin Folkes, Esq; President of the Royal Society, concerning the Action of Springs.*

S I R,

Presented April 12. 1744. I NEED not inform a Person so well acquainted with all the Branches of Mathematical Philosophy as yourself, that the Theory of Springs not only is of great Use in the more curious Parts of Mechanics, as the Structure of Watches, &c. but may give Light to many Operations of Nature, there being few Substances but what are endued with some Degree of Elasticity; and particularly the Bodies of Animals, and of Vegetables likewise, being known to consist, in a great measure, if not wholly, of Organs strongly elastic.

For which Reason it is not to be wondered at, that this Theory has engaged the Thoughts of several eminent

eminent Mathematicians of the last and present Age ; as Dr. *Hook*, Mr. *John Bernouilli*, M. *Camus*, &c.

But, as all that I have yet seen upon this Subject goes no further, than to compare the Effects of different Springs one with another, without shewing how the Effect of any of them may be reduced to, or compared with, that of any other natural Cause, I flatter myself, that the general Proposition I am going to lay down may merit your Attention, both on account of its Simplicity, and of its comprehending all possible Cases of a Body acting upon a Spring, or a Spring upon a Body, where no other Power intervenes ; and also of its reducing the Effect to that most known and simple one, the Effect of Gravity upon falling Bodies.

In order to which, to prevent any Misapprehension, it will be proper to fix the Meaning of such Terms as I shall have Occasion to make use of.

1. By a Spring, I mean a Body of any Shape perfectly elastic.

2. By the natural Situation of a Spring, I mean the Situation it will rest in, when not disturbed by any external Force.

3. By the Length of a Spring, I mean the greatest Length, through which it can be forced inwards. This would be the whole Length, were the Spring considered as a mathematical Line ; but in a material Spring is the Difference between the whole Length when the Spring is in its natural Situation, and the Length or Space it takes up when wholly compressed or closed.

4. By the Strength of a Spring, I mean the least Force or Weight, which, when the Spring is wholly compressed

compressed or closed, will restrain it from unbending itself.

5. By the Space through which a Spring is bent, I mean that Space or Length through which one End of the Spring is removed from its natural Situation.

6. By the Force of a Spring bent or partly closed, I mean the least Force or Weight, which, when the Spring is bent through any Space less than its whole Length, will confine it to the State it is then in, without suffering it to unbend any farther.

This being premised, I shall next, for the Foundation of what follows, lay down a Principle, which was verified by Experiment, in the Presence of our Royal Founder about 70 Years ago, by the famous * *Dr. Robert Hook*; and has been lately confirmed by the accurate Hand of our common Friend Mr. *George Graham*.

PRINCIPLE.

Ut Tensio, sic Vis: That is, if a Spring be forced or bent inwards, or drawn outwards, or any way removed from its natural Situation, its Resistance is proportional to the Space by which it is removed from that Situation.

Thus, if the Spring *CL*, (*Fig 1. TAB. II.*) resting with the End *L* against any immoveable Support, but otherwise lying in its natural Situation, and at full Liberty, shall, by any Force *p*, be pressed inwards, or from *C* towards *L*, through the Space of One Inch, and can be there detained by that Force *p*,

* *Lectures de Potentia restitutiva*, 1678.

the Resistance of the Spring, and the Force p , exactly counterbalancing one another ; then the Force $2p$ will bend the Spring thro' the Space of Two Inches, $3p$ thro' Three Inches, $4p$ thro' Four Inches, &c. the Space Cl (*Fig. 2.*), thro' which the Spring is bent, or by which the End C is removed from its natural Situation, being always proportional to the Force which will bend it so far, and will detain it so bent.

And if one End L be fastened to an immoveable Support, *Fig. 3.* and the other End C be drawn outwards to l , and be there detained from returning back by any Force p , the Space Cl , thro' which it is so drawn outwards, will be always proportional to the Force p , which is able to detain it in that Situation.

And the same Principle holds in all Cases, where the Spring is of any Form, whatsoever, and is, in any Manner whatsoever, forcibly removed from its natural Situation.

Here, Sir, I presume, you will think it material to take notice, that the elastic Force of the Air is a Power of a different Nature, and governed by different Laws, from that of a Spring. For supposing the Line LC , *Fig. 1.* to represent a cylindrical Volume of Air, which, by Compression, is reduced to Ll , *Fig. 2.* or, by dilatation, is extended to Ll , *Fig. 3.* its elastic Force will be reciprocally as Ll , *Fig. 2* and *3* ; whereas the Force or Resistance of a Spring will be directly as Cl .

I now proceed to my general Proposition, and its Corollaries ; in which if I shall happen at any time to express myself with less Accuracy, as in making Weights,

Times, Velocities, &c. to become promiscuously the Subjects of geometrical or arithmetical Operations, I desire, once for all, to be understood, not as speaking of those Quantities themselves, but of Lines, or Numbers, proportional to them.

THEOREM.

If a Spring of the Strength P , and the Length CL , *Fig. 4*, lying at full Liberty upon a horizontal Plane, rest with one End L against an immovable Support; and a Body of the Weight M , moving with the Velocity V , in the Direction of the Axis of the Spring, strike directly upon the other End C , and thereby force the Spring inwards, or bend it through any Space CB ; and a middle Proportional, CG , be taken between the Line $CL \times \frac{M}{P}$, and $2a$, a being the Height to which a heavy Body would ascend *in vacuo* with the Velocity V ; and, upon the Radius $R = CG$, be erected the Quadrant of a Circle GFA ; I say,

1. When the Spring is bent thro' any right Sine of that Quadrant, as CB , the Velocity v of the Body M , is, to the original Velocity V , as the Co-sine to the Radius: That is, $v = V \times \frac{BF}{R}$.

2. The Time t of bending the Spring thro' the same Sine CB , is to T the Time of a heavy Body's Ascending *in vacuo* with the Velocity V , as the corresponding Arch to $2a$: That is $t = T \times \frac{GF}{2a}$.

DEMON-

DEMONSTRATION.

1. While the Spring is bending thro' the Space CB , let the Space, thro' which it is at any time bent, be called x , τ the Time of bending it thro' the Space x , and v the Velocity of the Body at the End of the Time τ ; and let $CL = L$, $CB = l$.

Then, if p be the Force, with which the Spring, when bent thro' the Space x , resists the Motion of the Body; by Dr. *Hook's* Principle, $L : x :: P : p = \frac{Px}{L}$.

And since, in the Case of Forces that act uniformly, the Quantities of Motion generated are proportional to the generating Forces, and the Times jointly, if Mv be the nascent Quantity of Motion taken from the Body by the Resistance $\frac{Px}{L}$ in the nascent Time

$$\dot{\tau}, MV : -M\dot{x} :: MT :: \frac{Px\dot{\tau}}{L} \text{ or, } -\dot{v} = \frac{VPx\dot{\tau}}{MLT}.$$

Also, since, in the same Case of Forces acting uniformly, the Spaces are proportional to the Velocities, and the Times jointly, $x : 2a :: v\tau :: VT$, or $\tau = \frac{TVx}{2av}$.

$$\text{Therefore, } -\dot{v} = \frac{VPx}{MLT} \times \frac{TVx}{2av}, \text{ or, } 2v\dot{v} = -$$

$\frac{V^2Px^2}{MLa}$; and the Fluents of these Two Quantities are v^2 and $-\frac{V^2Px^2}{2MLa}$. But the former of these was V^2 , when x , and consequently, the latter was nothing;

thing; therefore $v^2 - V^2 = -\frac{V^2 P x^2}{2 M L a}$, or $v^2 = V^2 - \frac{V^2 P x^2}{2 M L a}$.

But, by the Construction, $\frac{2 M L a}{P} = R^2$; therefore, $v^2 = V^2 - \frac{V^2 x^2}{R^2}$, or, $v^2 = V^2 \times \frac{R^2 - x^2}{R^2}$; and, when x becomes equal to l , and v to v , $v^2 = V^2 \times \frac{R^2 - l^2}{R^2}$; and, by the Property of the Circle, $R^2 - l^2$ being equal to $B F^2$, $v^2 = V^2 \times \frac{B F^2}{R^2}$, or $v = V \times \frac{B F}{R}$. Q. E. D. 1°.

2. We have above, $\dot{\tau} = \frac{T V \dot{x}}{2 a v}$; and $v^2 = V^2 \times \frac{R^2 - x^2}{R^2}$; or, $v = V \times \frac{\sqrt{R^2 - x^2}}{R}$: Therefore, $\dot{\tau} = \frac{T V \dot{x}}{2 a} \times \frac{R}{V \times \sqrt{R^2 - x^2}}$, or, $\dot{\tau} = \frac{T}{2 a} \times \frac{R \dot{x}}{\sqrt{R^2 - x^2}}$.

Now let $C D$, Fig. 5. be equal to x ; and draw the Co-sine $D E$, the Radius $C E$, and the Perpendicular $e d = \dot{x}$: Then will the Triangle $D E C$ be similar to the nascent Triangle $d e E$; and consequently, $D E : d e :: C E : e E = \frac{C E \times d e}{D E} = \frac{R \dot{x}}{\sqrt{R^2 - x^2}}$.

Therefore, $\dot{\tau} = \frac{T}{2 a} \times e E$, and $\tau = T \times \frac{G E}{2 a}$. And when a becomes equal to $C B$, and τ to t , the Arch $G E$ becomes equal to the Arch $G F$: Therefore $t = T \times \frac{G F}{2 a}$. Q. E. D. 2°.

SCHOLIUM I.

Whereas I have represented the Spring as resting against an immoveable Support at L , it will, perhaps, be objected, That no Support can be really immoveable; since any Body, how great soever, may be moved out of its Place by the least Force. But this Objection may easily be removed, by supposing the Spring to be continued till it becomes of twice the Length $\dot{C}L$, and that a second Body, equal to M , strikes against the opposite End of the Spring with the same Velocity in a contrary Direction; in which Case the Point L will be perfectly immoveable.

SCHOLIUM II.

Under this Theorem are comprehended the Three following Cases:

In Case 1. The Spring is bent thro' its whole Length, or is intirely compressed and closed, before the moving Force of the Body is consumed, and its Motion ceases.

In Case 2. The moving Force of the Body is consumed, and its Motion ceases, before the Spring is bent thro' its whole Length, or wholly closed.

In Case 3. The moving Force of the Body is consumed, and its Motion ceases, at the Instant that the Spring is bent thro' its whole Length, and is intirely closed.

For this Reason, and in order to make the following Corollaries of more ready Use, I shall take the Liberty of distributing them into Three Classes, the first of which are as general as the Theorem itself, extending to all the Three Cases, but are more
par-

particularly useful in Case 1. The Second Class of Corollaries extend to both the Second and Third Case; but are more particularly useful in Case 2. The Third Class extend only to Case 3. and, by that means, are much more simple than either of the former.

CLASS I.

General Corollaries, but of more particular Use in Case 1; wherein the Spring is wholly closed, before the Motion of the Body ceases.

Coroll. 1. When the Spring is bent thro' any right Sine CB , *Fig. 4.* the Loss of Velocity is to the original Velocity, as the versed Sine to the Radius, or $V - v = V \times \frac{Gg}{R}$.

For, since $v = V \times \frac{BF}{R}$, $V - v = V - V \times \frac{BF}{R} = V \times \frac{R - BF}{R} = V \times \frac{Gg}{R}$.

Coroll. 2. When the Spring is bent thro' any right Sine CB , the Diminution of the Square of the Velocity is to the Square of the original Velocity, as the Square of that right Sine to the Square of the Radius, or $V^2 - v^2 = V^2 \times \frac{CB^2}{R^2}$.

For, since $v = V \times \frac{BF}{R}$, $v^2 = V^2 \times \frac{BF^2}{R^2}$, and $V^2 - v^2 = V^2 - V^2 \times \frac{BF^2}{R^2} = V^2 \times \frac{R^2 - BF^2}{R^2} = V^2 \times \frac{CB^2}{R^2}$.

Coroll. 3. When the Spring is bent thro' any Space l , the Velocity of the Body is equal to $V \times \sqrt{\frac{2MLa - Pl^2}{2MLa}}$, or to $V \times \sqrt{\frac{2Ma - pl}{2Ma}}$; and is proportional to $\sqrt{\frac{2MLa - Pl^2}{ME}}$, or to $\sqrt{\frac{2Ma - pl}{M}}$.

For,

For, since $v^2 = V^2 \times \frac{BF^2}{R^2} = V^2 \times \frac{R^2 - l^2}{R^2}$; if, for R^2 , we substitute its Value $\frac{2MLa}{P}$, we have $v^2 = V^2 \times \frac{2MLa - Pl^2}{2MLa}$, or $v = V \times \sqrt{\frac{2MLa - Pl^2}{2MLa}}$: And, as by Dr. *Hook's* Principle, $L : l :: P : p$, or $Pl = pL$, $v = V \times \sqrt{\frac{2MLa - pLl}{2MLa}}$, or, $v = V \times \sqrt{\frac{2Ma - pl}{2Ma}}$.

But $\frac{V}{\sqrt{a}}$, by *Galileo's* Doctrine, is a constant Quantity; and therefore v is proportional to $\sqrt{\frac{2MLa - Pl^2}{ML}}$, or, to $\sqrt{\frac{2Ma - pl}{M}}$.

Coroll. 4. The Time t of bending the Spring thro' any Space l , is proportional to the Arch GF divided by \sqrt{a} ; l being the right Sine of the Arch, and $R = \sqrt{\frac{2MLa}{P}}$, being the Radius.

For, by the Theorem, $t = T \times \frac{GF}{2a}$; and $\frac{T}{\sqrt{a}}$ is a constant Quantity.

Coroll. 5. The Diminution of the Product of the Weight of the Body into the Square of the Velocity, or (to use the Expression of some late Writers) the Diminution of the *Vis viva*, that is, $MV^2 - Mv^2$, by bending a Spring thro' any Space l , is always equal to $\frac{C^2 Pl^2}{2LA}$, or to $\frac{C^2 pl}{2A}$; where A is the Height from which a heavy Body will fall *in vacuo* in a Second of Time, and C is the Celerity gained by that Fall.

For, by *Coroll. 2.* $V^2 - v^2 = V^2 \times \frac{CB^2}{R^2} = \frac{V^2 l^2}{R^2}$; and R^2 , by the Construction, being equal to $\frac{2MLa}{P}$, $V^2 - v^2 = V^2 l^2 \times \frac{P}{2MLa}$.

But, by *Galileo's Theory*, $\frac{V^2}{a} = \frac{C^2}{A}$; therefore, $V^2 - v^2 = \frac{C^2 Pl^2}{2MLA}$ and $MV^2 - Mv^2 = \frac{C^2 Pl^2}{2LA} = \frac{C^2 pl}{2A}$.

Coroll. 6. The Diminution of the *Vis viva*, by bending a Spring thro' any Space l , is always proportional to $\frac{Pl^2}{L}$, or to pl : And, if either the Spring be given, or $\frac{P}{L}$ be given in different Springs, the Loss of the *Vis viva* will be as l^2 , or as p^2 .

For, by *Coroll. 5.* $MV^2 - Mv^2 = \frac{C^2 Pl^2}{2LA} = \frac{C^2 pl}{2A}$; and $\frac{C^2}{A}$ being a constant Quantity, $MV^2 - Mv^2$ is as $\frac{Pl^2}{L} = pl$: And, if $\frac{P}{L}$ be given, $MV^2 - Mv^2$ will be as l^2 ; or as $l^2 \times \frac{P^2}{L^2}$; or as $l^2 \times \frac{p^2}{l^2}$; or as p^2 .

Coroll. 7. The Loss of the *Vis viva*, by bending a Spring thro' its whole Length, or by wholly closing it, is equal to $\frac{C^2 PL}{2A}$, and is proportional to PL : And, if PL be given, the Loss of the *Vis viva* is always the same.

This

This is evident from *Coroll.* 5. and 6.; forasmuch as l is now equal to L .

CLASS II.

Corollaries of more particular Use in Case 2. ; wherein the Motion of the Body ceases before the Spring is wholly closed.

Coroll. 8. If the Motion of the Body cease when the Spring is bent thro' any Space l , the initial Velocity V is equal to $C l \sqrt{\frac{P}{2MLA}}$, or to $C \sqrt{\frac{pl}{2MA}}$.

For, by *Coroll.* 5. $V^2 - v^2 = \frac{C^2 Pl^2}{2MLA} = \frac{C^2 pl}{2MA}$.
And here, the Motion of the Body ceasing, $v^2 = 0$.
Therefore $V^2 = \frac{C^2 Pl^2}{2MLA} = \frac{C^2 pl}{2MA}$; or $V = C l \sqrt{\frac{P}{2MLA}} = C \sqrt{\frac{pl}{2MA}}$.

Coroll. 10. If the Motion of the Body cease, when the Spring is bent thro' any Space, l , the Time, t , of bending it, is equal to $1''$ of Time, multiplied by $\frac{m}{2} \sqrt{\frac{ML}{2PA}}$, or to $1'' \times \frac{m}{2} \sqrt{\frac{Ml}{2pA}}$, where m is to 1, as the Circumference of a Circle to the Diameter.

For, by the Theorem, $t = T \times \frac{GF}{2a}$; and, by *Galileo's* Theory, $\frac{T}{\sqrt{a}} = \frac{1''}{\sqrt{A}}$. Therefore $t = \frac{1''}{\sqrt{A}} \times \frac{GF}{2\sqrt{a}}$.

Also, by the Theorem, $v^2 = V^2 \times \frac{R^2 - l^2}{R^2}$; and therefore v^2 being now equal to 0, $R^2 = l^2$, and, *Fig. 6.* l becomes the Radius of the Circle; and l being likewise equal to the right Sine of the Arch GF , that Arch becomes a Quadrant, and is equal to $\frac{2 l \times m}{4}$. Therefore $t = \frac{1''}{\sqrt{A}} \times \frac{2 l m}{4 \times 2 \sqrt{a}}$, or $t = 1'' \times \frac{l m}{4 \sqrt{A} \sqrt{a}}$.

But l being equal to $R = \sqrt{\frac{2 M L a}{P}}$, $\frac{l}{\sqrt{a}} = \sqrt{\frac{2 M L}{P}}$; therefore $t = 1'' \times \frac{m}{4 \sqrt{A}} \times \sqrt{\frac{2 M L}{P}}$, or, $t = 1'' \times \frac{m}{2} \sqrt{\frac{M L}{2 P A}} = 1'' \times \frac{m}{2} \sqrt{\frac{M l}{2 P A}}$.

Coroll. 11. In the same Case, the Time of bending the Spring is proportional to $\sqrt{\frac{M L}{P}}$, or to $\sqrt{\frac{M l}{P}}$; and if $\frac{L}{P}$ be given, t will be as \sqrt{M} ; and, if both $\frac{L}{P}$, and also M , be given, t will always be the same, whatever be the original Velocity, or thro' whatever Space the Spring be bent.

Coroll. 12. If the Motion of the Body cease, when the Spring is bent thro' any Space l , the Product of the initial Velocity, and the Time of bending the Spring, or $V t$, is equal to $1'' \times \frac{m C l}{4 A}$; and is proportional to l , the Space thro' which the Spring is bent.

For, by *Coroll.* 8. $V = Cl \sqrt{\frac{P}{2MLA}}$, and, by *Coroll.* 9. $t = 1'' \times \frac{m}{2} \sqrt{\frac{ML}{2PA}}$; therefore, $Vt = 1'' \times \frac{mCl}{4A} \sqrt{\frac{MLP}{MLP}} = 1'' \times \frac{mCl}{4A}$; and, as $1''$, m , C and A , are given Quantities, Vt is as l .

Hence, any Two of the Three Quantities, V , t , and l , being given, the other is readily determined.

Coroll. 13. In the same Case, the initial Quantity of Motion, or MV , is equal to $Cl \sqrt{\frac{PM}{2LA}}$, or to $C \sqrt{\frac{PlM}{2A}}$.

For, by *Coroll.* 8. $V = Cl \sqrt{\frac{P}{2MLA}} = C \sqrt{\frac{Pl}{2MA}}$; wherefore $MV = Cl \sqrt{\frac{PM}{2LA}} = C \sqrt{\frac{PlM}{2A}}$.

Coroll. 14. In the same Case, MV is proportional to $l \sqrt{\frac{PM}{L}}$, or to \sqrt{PlM} , or to $\frac{Plt}{L}$, or to pt :

And, if $\frac{P}{L}$ be given, MV is as $l \sqrt{M}$, or as lt .

For, in the preceding *Coroll.* $\frac{C}{\sqrt{A}}$ is a given Quantity; and, by *Coroll.* 11. t is as $\sqrt{\frac{ML}{P}} = \sqrt{\frac{Ml}{P}}$.

Coroll. 15. If the Quantity of Motion MV bend a Spring of the Strength P , and Length L , thro' the Space l , and be wholly consumed thereby, no different Quantity of Motion equal to the former, as $n M \times \frac{V}{n}$, will bend the same Spring thro' the same Space, and be wholly consumed thereby.

For, by the preceding *Coroll.* if the Spring be bent thro' the Space l , and each of these Quantities of Motion be consumed thereby; $l \vee M : l \vee n M :: MV : n M \times \frac{V}{n}$. But $MV = n M \times \frac{V}{n}$; and therefore, $l \vee M = l \vee n M$, or $1 = n$, and $M = n M$, and $V = \frac{V}{n}$. Therefore the Quantity of Motion $n M \times \frac{V}{n}$ is not only equal to MV , but is composed of an equal Mass, and an equal Velocity.

Coroll. 16. But a Quantity of Motion less than MV , in any given *Ratio*, may bend the same Spring thro' the same Space l , and the Time of bending it will be less in the same given *Ratio*.

For, let 1 to n be the given *Ratio*; and let the lesser Quantity of Motion be $\frac{M}{n} \times n V$; which is to MV , as 1 to n . Then, by *Coroll.* 14. the Spring being given, $l \vee M : l \vee \frac{M}{n} :: MV : \frac{M}{n} \times n V = \frac{MV}{n}$. Therefore the Quantity of Motion $\frac{M}{n} \times n V$, being equal to $\frac{MV}{n}$, will bend the Spring thro' the same Space l .

Likewise, by the same Corollary, MV is as $l t$; and l being given, the Quantity of Motion is as t : Therefore the Time of bending the Spring will be less in the same *Ratio*, as the Quantity of Motion is less.

Coroll. 17. A Quantity of Motion greater than MV , in any *Ratio* given, may be consumed in bending the Spring thro' the same Space; and the Time of bending it will be greater in the same given *Ratio*.

This appears after the same manner as the preceding, by making n a fractional Number instead of a whole one.

Coroll. 18. If the Motion of the Body cease, when the Spring is bent thro' any Space l , the initial *Vis viva*, or MV^2 , is equal to $\frac{C^2 P l^2}{2LA}$; or to $\frac{C^2 p l}{2A}$: And $2aM = \frac{Pl^2}{L} = pl$.

For, by *Coroll. 8.* $V = Cl\sqrt{\frac{P}{2MLA}} = C\sqrt{\frac{pl}{2MA}}$,
or $V^2 = \frac{C^2 l^2 P}{2MLA} = \frac{C^2 pl}{2MA}$: Therefore $MV^2 = \frac{C^2 Pl^2}{2LA} = \frac{C^2 pl}{2A} = \frac{V^2 Pl^2}{2La} = \frac{V^2 pl}{2a}$.

Coroll. 19. In the same Case, the initial *Vis viva* is proportional to $\frac{Pl^2}{L} = pl$ and if $\frac{P}{L}$ be given, the *Vis viva* is as l^2 , or as p^2 .

For, in the preceding Corollary, $\frac{C^2}{A}$ being a given Quantity, the *Vis viva* is as $\frac{Pl^2}{L} = pl$; and, if $\frac{P}{L}$ be given, it will be as l^2 , or as p^2 ; so far as p and l increase in the same Proportion.

Coroll. 20. If the *Vis viva*, MV^2 , bend a Spring thro' the Space l , and be totally consumed thereby, any other *Vis viva*, equal to the former, as nn $M \times \frac{V^2}{nn}$, will bend the same Spring thro' the same Space, and be totally consumed thereby.

For, the Spring being the same, $\frac{P}{L}$ is given; and therefore by *Coroll. 19.* the *Vis viva*, which will

will be consumed in bending the Spring thro' the Space l , is as l^2 .

Coroll. 21. But the Time, in which the same Spring will be bent thro' the same Space, by the *Vis viva* $n n M \times \frac{V^2}{n n}$, will be to the Time, in which it is so bent by the *Vis viva* $M \times V^2$, as n to 1; n being any whole or fractional Number.

For, by *Coroll. 11.* since $\frac{L}{P}$ is given, the Time is as $V M$.

CLASS III.

Corollaries in Case 3. wherein the Motion of the Body ceases, at the Instant that the Spring is wholly closed.

Coroll. 22. If the Motion of the Body cease, when the Spring is bent thro' its whole Length, or is wholly closed, the initial Velocity V is equal to $C \sqrt{\frac{P L}{2 M A}}$.

For, by *Coroll. 8.* $V = C \sqrt{\frac{p l}{2 M A}}$; and l being now equal to L (*Fig. 7.*), p becomes equal to P ; and therefore $V = C \sqrt{\frac{P L}{2 M A}}$.

Coroll. 23. In the same Case, the initial Velocity V is proportional to $\sqrt{\frac{P L}{M}}$.

For $\frac{C}{\sqrt{A}}$, in the preceding Corollary, is a given Quantity.

Coroll. 24. In the same Case, if $P L$ be given, either in the same, or in different Springs, the initial Velocity V is reciprocally as \sqrt{M} .

This is plain from the preceding Corollary.

Coroll. 25. If the Motion of the Body cease, when the Spring is wholly closed, the Product of the initial Velocity, and the Time spent in closing the Spring, or Vt , is equal to $1'' \times \frac{m C L}{4 A}$; and is proportional to L , the Length of the Spring.

For, by *Coroll. 22.* $V = C \sqrt{\frac{P L}{2 M A}}$; and, by *Coroll. 10.* $t = 1'' \times \frac{m}{2} \sqrt{\frac{M L}{2 P A}}$: Therefore, $Vt = 1'' \times \frac{m C L}{4 A}$; and $1''$, m and $\frac{C}{A}$, being given Quantities, Vt is as L .

Coroll. 26. In the same Case, the initial Quantity of Motion, or MV , is equal to $C \sqrt{\frac{P L M}{2 A}}$

For, by *Coroll. 23.* $V = C \sqrt{\frac{P L}{2 M A}}$.

Coroll. 27. In the same Case, MV is proportional to $\sqrt{P L M}$, or to $P t$: And, if $P L$ be given, either in the same, or different Springs, MV is as \sqrt{M} .

This appears, partly, from the preceding Corollary, where $\frac{C}{\sqrt{A}}$ is a given Quantity; and; consequently, MV is as $\sqrt{P L M}$; and $P L$ being given, MV is as \sqrt{M} : And, partly, from *Coroll. 11.*; where t is as $\sqrt{\frac{M L}{P}}$, and, consequently, $P t$ is as $\sqrt{P L M}$.

Coroll. 28. In the same Case, if $\frac{P}{L}$ be given, either in the same, or in different Springs, the initial Quantity of Motion is as the Length of the Spring into the Time of bending it.

For, by *Coroll. 27.* MV is as $P t$; and, if P be as L , MV is as $L t$.

Coroll.

Coroll. 29. If the Quantity of Motion MV bend a Spring thro' its whole Length, and be consumed thereby, no other Quantity of Motion equal to the former, as $n M \times \frac{V}{n}$, will close the same Spring, and be wholly consumed thereby.

This is proved in the same manner as *Coroll. 15.* putting only L for l .

Coroll. 30. But a Quantity of Motion less or greater than MV , in any given *Ratio*, may close the same Spring, and be wholly consumed in closing it: And the Time spent in closing the Spring will be respectively less or greater, in the same given *Ratio*.

This is easily proved from *Coroll. 16.*

Coroll. 31. If the Motion of the Body cease, when the Spring is wholly closed, the initial *Vis viva*, or MV^2 , is equal to $\frac{C^2 P L}{2 A}$: And $2 a M = P L$.

For, by *Coroll. 22.* $V = C \sqrt{\frac{P L}{2 M A}}$, or $V^2 = \frac{C^2 P L}{2 M A}$, or $M V^2 = \frac{C^2 P L}{2 A} = \frac{V^2 P L}{2 a}$.

Coroll. 32. In the same Case, the initial *Vis viva* is as the Rectangle under the Strength and Length of the Spring.

For, by the preceding Corollary, $MV^2 = \frac{C^2 P L}{2 A}$, and $\frac{C^2}{A}$ is a given Quantity; wherefore MV^2 is as $P L$.

Coroll. 33. In the same Case, if $\frac{P}{L}$ be given, the initial *Vis viva* is as P^2 , or as L^2 .

This

This is evident from the preceding Corollary.

Coroll. 34. If the *Vis viva* MV^2 bend a Spring thro' its whole Length, and be consumed in closing it, any other *Vis viva* equal to the former, as nn $M \times \frac{V^2}{n \cdot n}$, will close the same Spring, and be consumed thereby.

This is evident from *Coroll. 32.*

Coroll. 35. But the Time of closing the Spring by the *Vis viva* $nn \cdot M \times \frac{V^2}{n \cdot n}$, will be to the Time of closing it by the *Vis viva* MV^2 , as n to 1.

For, by *Coroll. 11.* since the Spring is given, the Time is as \sqrt{M} .

Coroll. 36. If the *Vis viva* MV^2 be wholly consumed in closing a Spring of the Strength P , and Length L ; the *Vis viva*, $nn \cdot MV^2$, will be sufficient to close,

1. Either a Spring of the Strength nnP , and Length L .
2. Or a Spring of the Strength nP , and Length nL .
3. Or of the Strength P , and Length nnL .
4. Or, if n be a whole Number, the Number nn of Springs, each of the Strength P , and Length L , one after another.

For, $MV^2 : nn \cdot MV^2 :: PL : nn \cdot PL$; and therefore, by *Coroll. 32.* the *Vis viva*, $nn \cdot MV^2$, will close any Spring that has $nn \cdot PL$ for the Product of its Strength and Length. But $nn \cdot PL$ is composed either of $nn \cdot P \times L$, or of $n \cdot P \times n \cdot L$, or of $P \times nn \cdot L$.

Also the Loss of the *Vis viva*, in bending a given Spring, being always the same, by *Coroll. 7.* and the *Vis viva*, MV^2 being wholly lost in bending

a single Spring PL ; the Loss of the *Vis viva*, $nn MV^2$, in closing one such Spring, will be MV^2 ; and its Loss in closing a second such Spring, will again be MV^2 , and so on: Consequently, the Number nn of such Springs will be closed one after another, by that time the *Vis viva*, $nn MV^2$, is wholly consumed.

SCHOLIUM III.

If the Spring, instead of being at first wholly unbent, as we have hitherto consider'd it, be now supposed to have been already bent thro' some Space CB , before the Body strikes it; and the Velocity of the Body be required, after the Spring is bent thro' any further Space, BD , *Fig. 8.* this Case, as well as the Three other above-mention'd, will be found to come under our Theorem.

For, if v be the Velocity with which the Body is supposed to strike against the bent Spring at B , it is evident, that this may be consider'd, either as the original Velocity, or as the Remainder of a greater Velocity V , with which the Body might have struck upon the Spring at C , and which, upon bending the Spring from C to B , would now be reduced to v . For, in either Case, the Effect in bending the Spring from B to D , will be exactly the same.

In order, therefore, to determine this imaginary Velocity V , let a middle Proportional, BF , be taken between $CL \times \frac{M}{P}$, and 2α , α being the Height to which a Body will ascend *in vacuo* with the Velocity v ; draw BF perpendicular to CB , and, with the

the Radius CF , describe the Quadrant $CGFEA$. Then will our present Case be exactly reduced to that of the Theorem; CB, CD , representing the Spaces thro' which the Spring is bent; BF and DE the Velocities in the Points B and D ; GF and GE the Times of bending the Spring thro' the Spaces CB, CD ; and CG representing the imaginary Velocity V , with which the Body might have struck the Spring at C .

For, by the Theorem, $BF^2 : CG^2 :: v^2 : V^2$; and $v^2 : V^2 :: \alpha : a$. Therefore $CG^2 = BF^2 \times \frac{a}{\alpha}$. But $BF^2 = 2 \alpha \times \frac{LM}{P}$, by the Construction; and, consequently, $CG^2 = \frac{2 \alpha LM}{P} \times \frac{a}{\alpha} = \frac{2 a LM}{P}$, as in the Construction of the Theorem.

From this Case we shall draw a few Corollaries, as well for their Usefulness upon other Occasions, as to shew how the Theory of Springs may be safely applied to the Action of Gravity upon ascending or falling Bodies.

Coroll. 37. If the Body M , with the Velocity v , sufficient to carry it to the Height α , strike at B , upon a Spring already bent thro' the Space $CB = l$; and do thereby bend it thro' some farther Space $BD = s$; at the End of which Space, or at D , the Body has a Velocity sufficient to carry it to some Height, as ε ; then $\varepsilon = \frac{2 \alpha ML - P s \times 2 l + s}{2 ML}$.

For, by the Theorem, $\alpha : \varepsilon :: BF^2 : DE^2$, or $DE^2 = BF^2 \times \frac{\varepsilon}{\alpha} = \frac{2 \alpha ML}{P} \times \frac{\varepsilon}{\alpha}$ or $DE^2 = \frac{2 \varepsilon ML}{P}$.

Also, $\mathcal{D}E^2 + C\mathcal{D}^2 = CE^2 = CF^2 = BF^2 + CB^2$, that is, $\frac{2\epsilon ML}{P} + l^2 + 2ls + s^2 = \frac{2\alpha ML}{P} + l^2$; or $\frac{2\epsilon ML}{P} = \frac{2\alpha ML}{P} - 2ls - s^2$; or $2\epsilon ML = 2\alpha ML - Ps \times \overline{2l+s}$.

Coroll. 38. If the Motion of the Body cease upon bending the Spring thro' the Space $BD = s$, that is, if $\epsilon = 0$; then the Height to which the Body might ascend *in vacuo*, with the Velocity v , or $\alpha = \frac{Ps \times \overline{2l+s}}{2ML}$.

For, by the last, when $\epsilon = 0$, $2\alpha ML = Ps \times \overline{2l+s}$.

Coroll. 39. If p , the Force of the Spring when bent thro' the Space CB , be equal to M the Weight of the Body; the Height to which the Body would ascend *in vacuo* with the Velocity v , is to the Space thro' which it will bend the Spring, by striking upon it at B with that same Velocity, as $2l+s$ to $2l$, or $\alpha : s :: 2l+s : 2l$.

For, by the last, $\alpha = \frac{Ps \times \overline{2l+s}}{2ML}$; and $\frac{P}{L}$ being equal to $\frac{p}{l}$, $\alpha = \frac{ps \times \overline{2l+s}}{2Ml}$; and, if $p = M$, $\alpha = s \times \frac{\overline{2l+s}}{2l}$.

Coroll. 40. If $p = M$, and p do also continue constantly the same while the Spring is bending from B to \mathcal{D} (both which Suppositions are necessarily made in reducing the Action of a Spring to that of Gravity upon an ascending Body), the Spring must be of an infinite Length; and l , the Space thro' ... thro'.

thro' which it was bent before the Body struck it, must also be of an infinite Length; and the Space $B\mathcal{D}$, thro' which the Spring will be further bent, must be equal to the Height the Body can ascend to with the Velocity v , or $\alpha = s$.

For, by the last, when $p = M$, $\alpha : s :: 2l + s : 2l$; and the Resistances of the Spring at \mathcal{D} and B being respectively as $C\mathcal{D}$ and CB , that is, as $l + s$ and l ; since those Resistances are now supposed equal to one another, we must, upon that Supposition, consider $l + s$ as equal to l ; and adding l to each, $2l + s = 2l$, that is, l must be infinitely greater than s ; and then $\alpha : s :: 2l : 2l$, or $\alpha = s$.

SCHOLIUM IV.

In this Proposition, and all its Corollaries, except the Four last, we have considered the Spring as being, at first, wholly unbent, and then acted upon by a Body moving with the Velocity V , which bends it thro' some certain Space: But, as we suppose the Spring to be perfectly elastic, the Proposition and Corollaries will equally hold, if the Spring be supposed to have been, at first, bent thro' that same Space, and, by unbending itself, to press upon a Body at Rest, and thereby to drive that Body before it, during the Time of its Expansion: Only, V , instead of being the initial Velocity, with which the Body struck the Spring, will now be the final Velocity, with which the Body parts from the Spring when wholly expanded.

SCHOLIUM V.

If the Spring, instead of being pressed inwards, be drawn outwards by the Action of the Body, we need
only

only make L the greatest Length to which the Spring can be drawn out beyond its natural Situation, without Prejudice to its Elasticity, l any lesser Length to which the Spring is drawn outwards, P and p the Forces, which will keep it from restoring itself when drawn out to those Lengths respectively, and the Proposition will equally hold good: As it will also, if the Spring be supposed to have been already drawn outwards to the Length l , and, in restoring itself, to draw the Body after it: Only, in this latter Case, V , the initial Velocity in the Proposition, will now be the final Velocity, as in *Scholium IV*.

SCHOLIUM VI.

Our Proposition equally holds good, when the Spring is of any Form whatsoever, provided L be always understood to be the greatest Length it can be bent or drawn to from its natural Situation, l any lesser Length, and P , p , the Forces which will confine it to these Lengths. For Dr. *Hook's* Principle extends to Springs of any Form.

I have been at the Trouble of drawing so great a Number of Corollaries from this Proposition, because, in the Controversy about the Force of Bodies in Motion, I have observed both Parties to support their Opinion by Arguments taken from the Theory of Springs; and I was willing impartially to furnish them both with means to examine into the Truth or Falshood of one another's Reasonings. I had Thoughts myself of making use of some of these Corollaries for that Purpose, being far from thinking, that the Dispute is about Words only; but this Letter is already drawn out to too great a Length;
and

and before I have Leisure to write again, I may possibly be prevented by a better Hand, which, I hope, may put an End to a Dispute that has too long pester'd the Learned World.

But, in this, I shall be guided by your Judgment; and shall therefore, at present, take up no more of your Time, than only to profess myself,

Dear S I R,

*Your most affectionate Friend,
and most obedient Servant,*

Apr. 10. 1744.

James Jurin.

XI. D. Alberti Haller *Concil. Aul. & Archiatri Regis Britann. & Electoris Brunsvic. Prof. Anat. & Bot. Gottingensis, S. R. Ang. & Suec. Soc. Observatio de Ovarii Steatomate, & de Pilis ibidem inventis.*

Read April 12. 1744. **N**ON rarissimas esse hujusmodi historias non ignoro; & minus raras esse video quam e re esset generis humani, neque tamen vulgares esse, vel hæ ipsæ transactiones philosophicæ docent, in quarum fastos duo exempla inferuerunt D. *Samson & Tyson.*

Ancilla fuit, post longum morbum consumpta, triginta fere annorum, cujus cadaver in theatrum nostrum illatum est die 24^o *Januarii*, anno 1743.

Cum

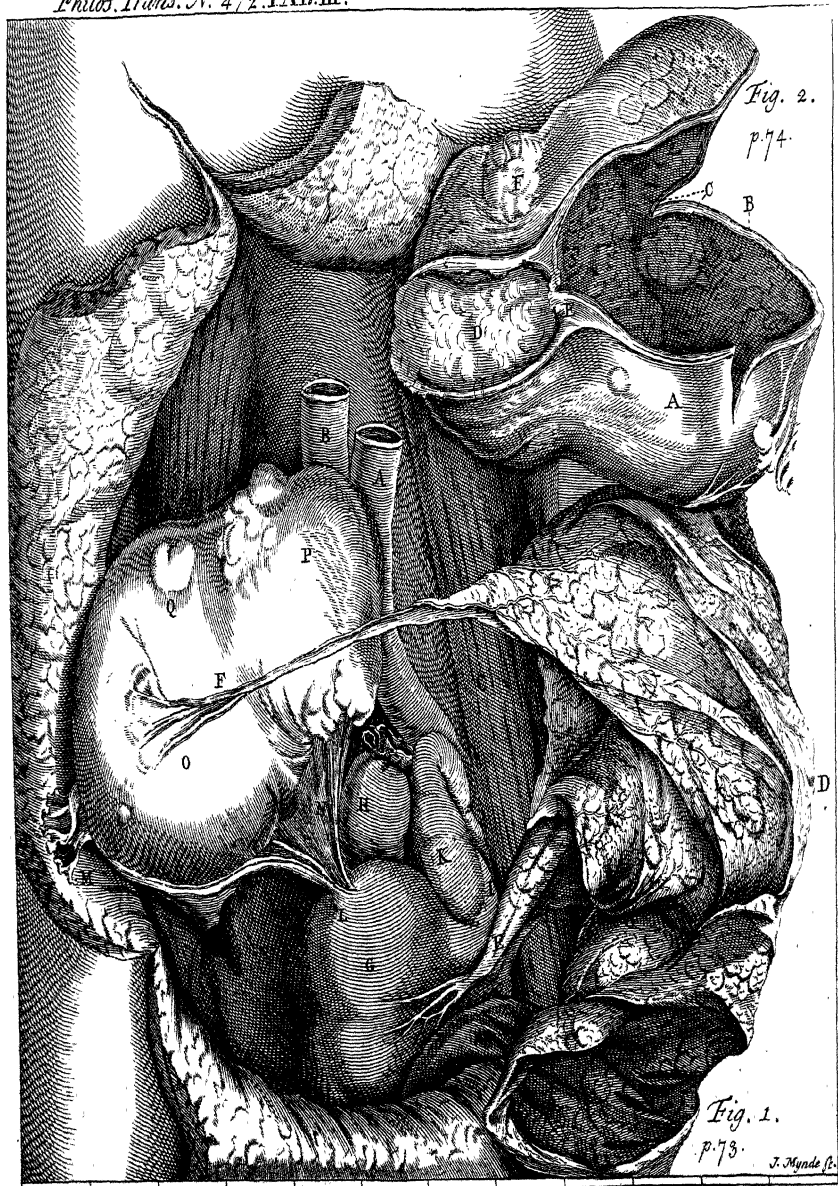
Cum corpus aperirem, continuo magnum in pelvi tumorem vidi, qui spem fecit graviditatis; sed frustra ea spes fuit; uterum enim inquirens suo loco reperi, tumores vero duos, modica convalle distinctos, in summa pelvi utero adjacentes.

Huic tumori adnatam reperi dextram tubam, ut pene media pars membranæ tubam efficientis in tunicam morboſi corporis innata & immerſa eſſet. Tuba cæca fuit. Huic porro tumori, deinde utero, duabus laciniis omentum adhæſit, ex *Hippocratis* antiquiſſima obſervatione. Sacci etiam morboſi tunicæ paſſim hydatides adhærebant. Nihil in ſiniſtro latere præter naturam ſe habuit.

Dimensiones omitto, quas icon exprimit. Tumorem, quem eſſe ovarium facile videbam, incidi; involucri craſſitiem reperi pene lineæ. Saccus erat undique cavus, ſep̄to quodam in duos loculos diviſus. In minori ſacculo materies melli ſimilis erat; ſed adipis tamen etiam ſimilis, in qua magna vis pilorum humanis ſimillimorum, fuſcorum, criſporum, pene biuncem longorum, temere hærebat. Melicerdis antiquorum certum exemplum naſtus, flammæ admovi mellæam maſſam; ignem ea continuo recepit, & adipis more deſlagavit.

In majori loculo alterius generis purulenta materies fuit lactis ſimilis; cui innatarent fruſta quaſi pulmonis, fuſca & friabilia.

Non arduum videtur definitu, ova duo, aut veſiculas duas, morboſe in hac femina intumuiſſe, & paulatim totam ovarii capacitatem ſibi uſurpâſſe, deletis reliquis quæ minime reſiſterent veſiculis. Id difficilius videtur, qua ratione in ovario nata ſit tanta vis adipis, ad unciam forte, ſi placuiſſet eximere omnem.



A Scale of 6 Inches.

omnem. Nam in sano Ovario, ne mica veræ pinguedinis adparet. Constat adeo, ubicunque cellulosa tela est, eo aliquando pinguedinem deponi posse, etsi minime vulgo soleat. Ita in scroto, in pene, adipem vidi; & novi repertum fuisse, in tenera illa cellulosa fabrica, quæ choroidem oculi interest, & scleroticam.

Deinde quæri potest, quomodo pili nasci potuerint, absque ulla vel cute, vel membrana, in qua radicati fuerint? Non ignoro etiam alibi pilos, etsi cutem perforant, in pinguedine tamen veras radices habere, quod in scroto & pube valde manifestum est. Sed id difficilius videtur, quomodo in loco ab epidermide remoto, involucrum tamen simillimum & corneum, & pellucens, in his morbosis pilis ortum sit? & annon inde labefactetur vulgaris sententia, crustam pilorum a cuticula esse; & demonstretur, corticem verum pili ab ipso cortice bulbilli continuari? Nam hos pilos foetus destructi reliquias fuisse non videtur: etsi minime ignorem in aliis, Tysonique imprimis casu fuisse: ibi enim cum pilis dentes etiam in ovario fuerunt. In nostra enim femina nihil quidquam de solidioribus corporis partibus, superstes factum esse, pilosque, adeo evidenter molliores, solos vim eam effugisse, quæ ossa dentesque destruxerat, id quidem non fit probabile.

Figurarum Explicatio.

TAB. III. Fig. I.

A, Arteria Aorta super pelvem.

B, Vena cava.

C, Psoas sinister.

K

D, Omentum.

- D*, Omentum.
E, Ejus adhæſio ad uterum.
F, Altera adhæſio ad ovarium morboſum.
G, Uterus.
H, Inteftinum rectum.
I, Tuba ſiniſtra.
K, Ovarium ſiniſtrum obiter indicatum.
L, Tuba dextra.
M, Ejus lacinia.
N, Ligamentum ovarii dextri.
OP, Ovarium morboſum.
O, Saccus major inferior.
P, Saccus minor.
Q Q, Hydatides externæ ſuperficiæ adnatæ.

Fig. II.

- A*, Saccus eſt morboſus ſeorſim pictus.
A, Loculus major apertus.
B, Craſſities morboſa membranæ ovarii.
C, Materiæ purulentæ pars aliqua ſuperſtes, poſtquam major pars effluxerat.
D, Loculus minor, in quo mellea pinguedo et pili.
E, Septum loculos ſeparans.
F, Hydatides.

Totus tumor cum utero in theſauris anatomicis
 Academiæ conſervavi.

XII. *A Catalogue of the FIFTY PLANTS from Chelsea Garden, presented to the ROYAL SOCIETY by the Company of Apothecaries, for the Year 1741. pursuant to the Direction of Sir Hans Sloane, Bart. Med. Reg. & super Soc. Reg. Præf. By Joseph Miller, Apothecary, Hort. Chels. Præf. ac Prælec. Botan.*

Read April 19. 1744. 951. **A** Absinthium Ponticum Galeni.
Ger.

952. Absinthium Tanacetii folio odoratissimum.
Amm. p. 142.

953. Alcea tenuifolia crispa. *J. B.*

954. Amaranthus maximus Offic. purpureus major.

955. Amaranthoides Lychnidis folio capitulis argenteis. *Tourn.*

956. Idem capitulis purpureis.

957. Anisum Offic. herbaris. *C. B.*

958. Asparagus sylvestris tenuissimo folio. *C. B.*

959. ——— aculeatus spinis horridus. *Ibid.*

960. Betonica Alpina incana purpurea. *Barellier. Icon.*

961. Bellis spinosa foliis Agerati. *C. B.* Santolina spinosa Agerati foliis. *Tourn.*

962. Bugula orientalis flore inverso cœruleo. *Tourn.*

963. Citrullus, five Anguria, vulgo. *Park.*

964. Carthamus Africanus folio Ilicis, flore aureo.
Boerhaave.

965. *Conyza humilior*, *Linariæ folio*, floribus luteis umbellatis *Amm.* 141.
966. *Cyanus Turcicus*, seu *orientalis odoratus major*. *Park.*
967. *Dictamnus montis Sipyli*. *Sir George Wheeler.*
968. *Elatarium*, *Tourn.* *Cucumis asininus.* *Ger.*
969. *Euonymo adfinis Æthiopica*, fructu globofo, *Salicis folio.* *Plukn.*
970. *Guajacana Virginiana Pisbamin dicta.* *Park.*
971. *Jacea non ramosa tuberosa radice latifolia.* *Banister. Pluk.*
972. *Idem angustifolia, &c.* *Ibid.*
973. *Jacobæa Ætnica*, *Chœnopodii folio.* *Hort. Cathol.*
974. *Leonurus Africanus*, *Sideritidis folio*, floribus *Phœniceis.* *Boerb.*
975. *Limonium maritimum majus.* *C. B.*
976. ———— *lignosum*, *Bellidis folio.* *Ibid.*
977. *Lupinaster floribus purpureis*, *siliquis minoribus.* *Amm. p. 147.*
978. *Marum vulgare.* *Park.* vulgo *Mastichen* redolens. *C. B.*
979. *Mentha verticillata* *Ocimi odore*, venis luteis. *Ind. Hort. Chelf.*
980. *Meum foliis Anethi.* *C. B.* — *vulgatius.* *Park.*
981. *Mimosa folio lato Sennæ spinosa.* *Boerb.*
982. *Myrtus*, foliis odore nucis *Moschatae.* *Schyl. Cat.*
983. ———— *latifolia Boetica*, foliis confertim nascentibus. *C. B.*
984. ———— *flore pleno.* *Cornuti.*
985. *Nigella Cretica semine aromatico.* *C. B.*
986. *Olea vulgaris sativa.* *C. B.*
987. *Olea*

987. *Olea maxima Hispanica.* C. B.
 988. — *minor Lucensis, fructu odorato.* *Ibid.*
 989. *Onagra frutescens argentea angustifolia.* *Ind.*
Hort. Cbels.
 990. *Pisum cortice eduli.* *Tourn.*
 991. *Pseudo-dictamnus acetabulis Moluccæ.* C. B.
 992. *Ptarmica flore pleno.*
 993. *Ricinus humilis, folio subrotundo, flore fructu-*
que conglomerato. *Houft.*
 994. *Rubia procumbens hexaphylla purpurea.* H.
L. B.
 995. *Serpyllum odoratissimum glabrum, longiore*
folio. *Amm.* p. 52.
 996. *Stoechas citrina angustifolia.* C. B.
 997. *Tithymalus Juniperi folio.* *Boccon.*
 998. *Verbena angustifolia.* C. B.
 999. *Veronica spicata latifolia major.* *Park.*
 1000. *Veronica spicata angustifolia.* *Ger. Park.*

XIII. *An easy Method of procuring the true*
Impression or Figure of Medals, Coins, &c.
humblly addressed to the ROYAL SOCIETY:
By Henry Baker, F. R. S.

Read April 19. 1744. **I**T must, I believe, be thought an agree-
 able and useful Thing, to be able to
 print off an exact and fair Picture, or Representation,
 upon Paper, of any Medal, Coin, or Seal, one may
 happen to get the Command of for a few Minutes,
 without doing it the least Injury, and with very
 little either of Expence or Trouble.

A Con-

A Contrivance for this Purpose is what I now humbly take the Liberty to lay before you; assuring myself, that whatever can prove really useful, will not be the less regarded by this Illustrious Society for being plain and easy.

The first Step is, to take a perfect and sharp Impression, in black Sealing-wax, of the Coin or Medal you desire the Picture or Figure of: When this is done, the chief Trouble is over, and the rest of the Operation may be executed at Leisure.

Cut the Wax away round the Edges of the Impression, with the Point of a Penknife, or a Pair of sharp Scissars; and, having ready a Preparation in Gum-water of the Colour you would have the Picture, spread your Paint upon the wax Impression with a small Hair-pencil, observing to work it into all the sinking and hollow Places, those being the rising or projecting Parts of the Medal, and what only are necessary to be laid over with the Colouring; for it must be intirely taken away from every other Part before we can proceed.

The Way of getting off the Paint from the Places where it should not be, is, to moisten your Forefinger a little, but not too much, with Spittle or Water, and pass it gently, but nimbly, over the Surface of the wax Impression; wiping it each time upon a Cloth or Handkerchief, till you perceive all the rising Parts of it perfectly fair and clean, and the Letters and sinking Parts of it only coloured.

This done, take a Piece of very thin Post-Paper, a little larger than the Medal; wet it in your Mouth, or with Water, till it be moistened quite through, but let not any Water hang upon it: Place it on

the wax Impression, laying on the Back of the Paper Three or Four Pieces of thick woollen Cloth, or Flannel, about the Size thereof.

I should premise, that you must have a Couple of flat smooth iron Plates, about Two Inches square, and One Tenth of an Inch in Thickness. The wax Impression must be placed, with its Face upwards, on the Middle of one of these Plates, before you spread the Paper and Flannels on it, and the other Plate must immediately be laid over them : Then, holding all tight together, put them carefully and evenly into a little Press, made of two iron Planks about Five Inches and half long, One Inch and half wide, and half an Inch in Thickness (shaped like what Bookbinders use of a large Size in Wood), having a Couple of long male Screws that run through them, with a turning female Screw on each to force the Planks together : And these female Screws must have strong Shoulders, whereby to work them. But all this will be comprehended better by the Figure hereto annexed. *See TAB. I. Fig. 4.*

Things being thus adjusted, hold the Press in your Left-hand, and, with a little Hammer, strike first on the Shoulders of one Screw, and then on the Shoulders of the other, to bring the Planks together parallel, and render the Pressure every-where alike ; unless you find it requisite to give more Force to one Side than the other, which these two Screws will put in your own Power.

The Press opens again, by a Stroke or two of the Hammer, the contrary Way, on the Shoulders of the Screws : and then you will find a true and fair Picture neatly printed off ; which (if any Deficiencies appear :

appear therein) you may easily repair, when dry, with a Hair-pencil, or a Pen, and a little of the same Colour.

If your Paper does not soak in the Moisture well, by being over-sized, it is necessary to wet the Flannels, or the Paper will not come off strongly enough colour'd: And, if the *Relievo* of your Medal be very high, it is best to put a little Cotton immediately upon the Back of the Paper, between that and the Flannels, that the Paper may be duly pressed into the deep Hollows of the wax Mould.

This Method is very easy and ready for taking the Picture of a Medal in any Colour: But, if you desire a *Relievo* only, without any Colour, the Way is abundantly shorter; for nothing then is necessary, but to place a Piece of Card, or white Pasteboard, well soaked in Water, on the wax Mould, without any Colouring, and letting it remain in your Press a few Minutes, the Business is done at once.

As it is plain, from what has been said, that the whole Success depends on the Goodness of the wax Impression or Mould, I shall lay down some Rules for taking it, which much Experience has taught me.

1. The Wax must be very fine; or it wants a proper Hardness, and the Impression will not prove sharp.
2. It must be spread wider than the Medal, and of a Thickness in proportion to the *Relievo* of it.
3. The Medal must be clapped on when the Wax has a right Degree of Heat: For, if it be too hot, the Medal is apt to stick; and, if too cold,

cold, no good Impression can be taken. I observe the best Time to be, just after the Wax ceases to work up, and have little Bubbles in it.

4. Take not the Impression on a Table, or any hard Body, without a Sheet or two of Paper, or, what is better, a woollen Cloth, underneath; for that gives way to the Pressure and Form of the Medal, which hard Bodies will not do.
5. The Medal should be squeezed down equally hard on every Side, and the Pressure continued till the Wax is near cold: For, if you lift up the Medal while the Wax remains hot, the rising Parts, being still soft, sink down, and become much less sharp.
6. The best Paper for your wax Impressions is white Pasteboard, Card-paper, or some other thick Paper.

The Pictures may be coloured as every one fancies. I have done them in most Colours, but think a Red the best; which was the Reason I advised the Impression to be taken in black Wax; since the Wax and Paint must be of different Colours, or it will be impossible to distinguish when the Colour is laid on properly, or rightly cleared away. Therefore, if the Pictures are chosen in Black and White, to resemble Copper-Plates, the Wax must not be black, but red.

The red Colouring I use is a Mixture of Lake and Vermilion, which works off more kindly than either of them alone.

Gamboge makes a good yellow Print, and appears very lively in the Day-time, but can scarce be seen by Candle-light: Mixed with Carmine, it affords a much better Colour.

Burnt Umber affords a Brown; but is more agreeable, if tintured with a little Lake.

Blue may be composed of Verditer and Indico; but is troublesome to clear off; and, after all, has but an indifferent Effect.

Green is likewise, in my Opinion, not very agreeable; but, if desired, Sap-Green serves the Purpose best.

Indian Ink makes the best Black; and affords Pictures very like those from Copper-Plates.

All these Colours, except Gamboge, Sap-Green, and *Indian Ink*, must be ground extremely fine, with Gum-water of an exact Strength: For, if there be too much Gum, the Colouring will not easily be cleared away from the wax Impression, nor readily come off upon the Paper; and, if too little, every Touch will spoil your Picture, after you have worked it off. Some few Trials will teach the proper Temper it ought to have.

If you would have the Hair, Face, Robes, or any Parts of the Print to be of different Colours, you must spread them so on your wax Impression. I have worked off many in that Manner, which I have here to shew; but, as it is much more troublesome to lay on the Colours thus, and requires a good deal of Finishing with the Pencil afterwards, I think it also less proper for the Picture of a Medal, than one single Colour.

I am not unacquainted with many ingenious Inventions for taking off Medals, in Sulphur, Plaister of *Paris*, Paper, &c. ; but, since a Mould must be formed for each of these, either of Clay, Horn, Plaister of *Paris*, or some other Materials, which requires a great deal of Trouble and Time to form, I believe this Way will be judged abundantly more convenient: And taking Impressions on Paper from the Medals themselves, by passing them through the Rolling-Press, and colouring them afterwards, is not only much more tedious, ineffectual, and less practicable (as a Gentleman cannot manage a Rolling-Press), but does really a great deal of Injury to the Medals, by impairing the Sharpness of their most delicate and expressive Strokes; as I myself have found by repeated Experience.

But Wax is always ready, and hurts not the finest Medal in the least Degree; and, however brittle it may be supposed, the Moulds made thereof resist the Force of downright Pressure, almost as effectually as if they were made of Steel; and might serve to take off a thousand Impressions, were they not apt to crack, and the Marks of those Cracks to render what are taken from them afterwards, not quite so elegant: But each Mould will usually afford three or four good Impressions, either coloured or plain. And, when they are done with, if the Paint is washed off clean, the same Wax may be melted and employed again several times over.

It is evident, that Impressions taken thus, must be exactly what the Medals are from whence we take them, and that any-body who can borrow Medals from his Friends, may, in this manner, at the

Expence of a little Pains, procure a noble Collection of genuine Prints or Medals, which may be placed in Books, in orderly Series, and moved from one Leaf to another at Pleasure, if they are only pasted down by little Slips of Paper left round the Edges for that Purpose. I flatter myself therefore, that the Usefulness of this Contrivance will not be slighted, on account of its being so plain and obvious, that every Gentleman will wonder himself did not hit on it; since Discoveries that are most easy, and, consequently, may be practised by every body, however simple and void of Invention they may appear, are really in themselves most valuable. As a Proof whereof, I shall only instance one Art (from which this, in some sort, is borrowed); I mean the Art of Printing; the most happy Discovery, that, perhaps, was ever made by Man! yet, seemingly, so easy, and what the Antients in their Seals approached so near to, that it is extremely surprizing it was not found out many Ages before it was.

Twenty-five Years ago and better, I first, accidentally, thought on and practised the Method here described: And though, since that time, I have taken off many Hundreds of Coins and Medals, for myself and Friends, I have hitherto been so far from disclosing it to any body, that, on the contrary, I have endeavoured to conceal it, by pressing the Pictures flat, that the rising Parts might give no Hint of so easy a Contrivance. But, at length, considering, that it may promote the Knowledge of Medals, whereby many Facts in History may be explain'd, that any Gentleman may divert himself by doing it, and that, possibly, it may prove acceptable to the Curious, I do

do myself the Honour to present it to this Society. And, in order to make it better understood, I subjoin a Drawing of the Press I use in its full Size; see TAB. I. *Fig. 4.* and have brought the whole *Apparatus* (the Expence whereof is not above four Shillings) to shew the Manner of employing it: I shall be also ready to give farther Information or Assistance to any Gentleman of this Society, who may desire or stand in need thereof.

I have likewise added to this Account, as Specimens, both the Picture and the *Relievo* of that Medal of Gold, which this Society bestows yearly, in consequence of Sir *Godfrey Copley's* Benefaction, as the *Premium* of some new Experiment, or useful Observation lately laid before them. By the way of Fancy, I have also placed with them the reverse Side of an Half-crown, and of a Silver Medal that came to Hand, taken off in Leather; and am ready to shew great Numbers of other Specimens.

And now, Gentlemen, to conclude, If I have intruded on your Time too long, or said too much in behalf of what has often afforded me much Pleasure, and, I think, capable of doing the same to others, I hope to obtain your Pardon; and am,

Your most obedient humble Servant,

London, April 19. 1744.

H. Baker.

N. B. Instead of the Water-Colours herein mentioned, I have lately made use of the Mixture (both Red and Black) which is usually empl. yed by the Rolling-Press Printers in working off Copper-Plates, with very good Effect: but when this is used, after it has been wiped off with a linen Cloth, it is necessary to clear it perfectly from the Ground of the wax Impression, by rubbing one of your Fingers on a Piece of Whiting, and passing it two or three times over your Impression, in the same manner as Copper-Plates are cleared.

XIV. Ob²

XIV. *Observations on the Manna Perficum :*
By John Fothergill, M. D. Licentiate of
the College of Physicians, London.

Read April 26.
1744; but here printed
with Additions.

IT does not appear very plain, from any thing that has occurred to me in the Writings of the elder *Greek* Physicians, that they were much, if at all, acquainted with any Substance that now goes under the Name of *Manna*. They had the Term, but applied it to a Subject very different from what we do at present. The Ὑπόσεισμα τῆς Λιβανώλης, or the *Micæ thuris concussu elisæ*, the Bits broke off from the *Olibanum* in Carriage, was the Substance they knew by that Name.

If the *Arabians* did not first of all introduce some Kind of purging *Manna* into Practice, they at least render'd the Use of this Drug more common and extensive. Their Country afforded several Species of it; which being familiarly known, is, perhaps, the Reason why no Descriptions have been left sufficiently clear, whereby to distinguish them from each other. Whoever has consulted the *Arabian* Writers, or the best of their Commentators, will allow that their Accounts are defective.

That they had three Sorts of this Drug, distinguished by the Names of *Manna*, *Tereniabin*, and *Siracost*, is certain; but whether these are now known, or by what Appellations, has been very much disputed.

Rawwolf, in his Itinerary published by *Ray*, and *Tournefort*, in his Voyage to the *Levant*, have given the clearest Intimations of any Writers that I know of:

of: If to these we add *Clusius*, we have all that we are to expect of Certainty, amongst those who have mention'd it; they being Eye-witnesses of what they wrote: Yet the Descriptions of these are either so imperfect or unknown, that a very late Writer upon the *Materia Medica* (a), either not having seen or understood them, has fallen into a Mistake about the *Manna Arabum*, and his Authority may perhaps mislead others.

My worthy Friend *Peter Collinson*, having procur'd a Sort of *Manna* from a Gentleman at *Petersburgh*, under the Title of *Manna Persicum ex planta Al-Hagi Maurorum*, was pleas'd to favour me with a Specimen of it: I consulted the principal Writers on the *Materia Medica*; and, finding their Opinions to be frequently opposite, and their Accounts in general perplexed, I imagined it would not be unacceptable to the Curious, to have such a Description of this Species, as would probably make it clear, that we have one Kind of the *Manna Arabum* still extant, viz. the *Terniabin*, and prevent any future Mistakes about it.

The Specimen of *Manna* that now lies before me, appears, at first Sight, to be a dirty reddish-brown colour'd mixed Mass; which, upon a nearer View, is found to consist of,

- i. A great Number of globular, crystalline, almost pellucid Bodies, of a yellowish-white Colour, and different Sizes; the biggest not much exceeding

* Geoffroy *Traçat. de Mat. Med.* Vol. II. p. 586, &c.

- a large Coriander-feed, or a very small Pea: They differ from Grains of Mastich, in being more upon the reddish Cast; but in Figure and Transparency it varies not much.
2. Some small Sticks like Prickles, and others like Footstalks of Leaves or Fruit.
 3. A few narrow-pointed firm small Leaves.
 4. A large Quantity of long reddish-colour'd Pods, of a sweetish gelatinous Taste, containing from one to six or seven hard, irregular, somewhat Kidney-like Seeds, which to the Taste are very acerb. And,
 5. Some Sand and Earth. Four Ounces of the *Manna*, dissolved in warm Water, left one Ounce or something more of these in the Filtre.

The Globules (N^o 1.) are hard, and break between the Teeth like Sugar-candy; they are of a pleasant sweet Taste, with much less of the *Manna* Relish than the *Calabrian*; but with enough to discover to what Family this Substance belongs.

The Sticks, Leaves, Pods, &c. seem to be Parts of the Plant that produces the *Manna*. Some of the Seeds have been sown, and proved so fresh as to afford some Plants of the *Alhagi*.

About the Year 1537, when *Rawwolf* wrote his Itinerary, it appears, that large Quantities of this Kind of *Manna* were brought from *Persia* to *Aleppo*, where it was then known under the Name of *Trunschibil* or *Trunschibin*; a Corruption, doubtless,
of

of the antient *Terenjabin*; or, as it ought to be wrote, according to *Deusingius* *, *Terengjabin*.

Rauwolf informs us, that this Species of *Manna* was gather'd from the *Alhagi*; a Plant which is minutely described by *Tournefort* †, who also confirms the Account which *Rauwolf* had long before given, with the following Particuiars :

“ It is chiefly (says he) about *Tauris*, a City in
 “ *Persia*, that it is gather'd, under the Name of *Trun-*
 “ *gibin* or *Terenjabin*, mention'd by *Avicenna* and
 “ *Serapion* : Those Authors thought it fell upon
 “ certain prickly Shrubs; whereas it is only the
 “ nutritious Juice of the Plant.” — He adds, “ That,
 “ during the great Heats, you perceive small Drops
 “ of Honey upon the Leaves and Branches of
 “ these Shrubs; these Drops harden, in Grains about
 “ the Bigness of Coriander-seeds : They gather those
 “ of the *Alhagi*, and make them into reddish Cakes
 “ full of Dust and Leaves, which alter the Colour,
 “ and lessen its Virtue. This *Manna* is much infe-
 “ rior to the *Italian*. The ordinary Dose is from
 “ 25 to 30 Drams.”

Clusius ‡ informs us, that the *Terniabin* of the modern *Arabs* is gather'd from a prickly Shrub, such as the *Alhagi* is described to be. *Avicenna* **, according to his present Translation, tells us, that the

* *Deusingius Tract. de Manna & Sacch.* p. 11.

† *Tournefort's Voyage to the Levant*, Vol. I. p. 247, 248.

‡ *Clus. Exotic.* Vol. II. p. 164.

** *Avicen. Oper.*

Tom. I. p. 404.

Tereniabin falls *super Lapides*; but * *Deusingius* says that it ought to be read, *super Alhagi*; and that his Translators were led into this Mistake, from the Resemblance betwixt *Al-Hhagier* (the Word in the *Arabic* Text, and which signifies a kind of thorny Plant, such as the *Alhagi* is said to be) to *Al-Hagio*.

It is therefore evident, that the *Manna Persicum*, now before us, is the *Tereniabin*, *Terenjabin*, *Terræjabin*, or more properly, the *Terengjabin*, of the old *Arabians*, and of *Clusius*; the *Trungibin*, or *Trunfchibil*, of the later, of *Rawwolf* and *Tournefort*; very probably, the *Manna mastichina orientalis* of *Matthiolum* and *Bauhine*; as it is the *Mastichina* and *Alhagina* of *Geoffroy*; tho' this Author makes the *Tereniabin* a Species of *Liquid Manna* †, in Complaisance to his Countryman *Bellonius*; who, tho' in general a diligent Observer, yet, in this Case, was misled by the *Caloyers*, or Monks of *Mount Sinai*.

Bellonius says, in his Observations ‡, and more largely in his Treatise *de Arboribus perpetua Fronde virentibus*, that these *Caloyers* collect a kind of liquid *Manna*, which they call *Tereniabin*; that this Species was known in the Shops at *Cairo* by the same Name; and that this is the *Mel ruscidum* of *Galen*, and the *Mel cedrinum* of *Hippocrates*.

I think it is very plain, that *Bellonius* was mistaken in the first Part of his Assertion, from what

* *Traët. de Manna*, p. 19.
Tom. II. p. 587.

† *Traët. de Mat. Med.*

‡ *Bellonii Observ. apud Clus.* p. 129.
has

has already been advanced. The *Caloyers* told him, that they called it *Tereniabin*; and he takes it for granted, that it was the *Tereniabin* of the old *Arabians*, contrary to their own Accounts, supported by the Testimony of their Successors, who are known to vary as little as any People whatever from the Traditions and Customs of their Predecessors, and still retain a great many of their Appellations. For Proof of this, I need only refer to the Accounts which *Rawwolf*, *Tournefort*, and Dr. *Shaw*, give us of those People.

That this liquid *Manna* was the *Δροσόμελι*, seu *Ἀερόμελι*, of *Galen* *, and the *Μέλι κέδεινον*, of *Hippocrates* †, (supposing there is no Mistake in the Text), seems very probable. The Description which *Galen* has left of the *Mel roscidum*, and the Manner of collecting it on *Mount Sinai* in his Time, tallies exactly with *Bellonius's* Account; and thus far, I believe, all Authors agree: But that the Virtues of *Manna* were known so early as in the Times of these Two Authors, will be difficult to prove.

Galen takes notice of this *Mel roscidum* more as a Curiosity, than a Medicine. He no-where, that I know of, mentions its Use, or describes its Qualities: He introduces the Account of it with a *Memini aliquando*, and says, that the *Mel roscidum* was rarely met with in his Country, but was gather'd at *Mount Sinai* every Year: And, indeed, from the Manner

* *Galen. de Alem. Facult. L. III. c. 39. de Ulceribus*, p. 876. Edit. Foesii.

† *Hippocrat.*

in which it is spoken of by an old *Greek* Writer in *Atheneus*, as cited by *Salmasius*, it would seem, that it was only used for Pleasure, as an agreeable Sweet, *Melle ipso suavius*; and, probably, continued to be of no other Use. *Mesue* tells us*, that *Galen* mixed *Manna* with *Scammony*. In the spurious *Piece de Dynamis* ascribed to *Galen*, *Scammony* is ordered to be mixed with Honey; but never once mentions *Manna* in any of his extant Writings. As *Galen* is known to be very minute in his Account of the *Materia Medica* of that Time, his Silence is a strong Argument against the Supposition, that even the *Mel roscidum* was in Pharmaceutic Use, much less any other Species of *Manna*.

If *Galen* was unacquainted with this Substance, it is very probable, that *Hippocrates* was so likewise; since a Drug that must have made a considerable Figure in his *Materia Medica*, would not have soon been struck off the List, or dropp'd into Oblivion and Disuse.

But how shall we get rid of the *Μέλι κέδρινον*; the Name sufficiently intimating what Substance was intended? Perhaps *Foesius's* Suggestion may help us. He thinks, that the Words might have been read with a Comma intervening, whereby we should have had Two distinct well-known Substances, Honey, and Resin of the Cedar; Two Simples that were then, and continued long after, in familiar Use; instead of one, which he mentions no-where else, and seems to be unknown some Ages after.

* *Mesue de Simpl.* c. 8.

Upon the whole, I have not hitherto met with Evidence sufficient to induce me to believe, that either the *Mel roscidum*, or any Kind of *Manna*, was in common medical Use either with *Hippocrates* or *Galen*. *Actuarius* mentions it once *, and, as I know of, only once: He makes it a Purgative, and to be somewhat stronger than *Cassia*.

It is now pretty generally known, that the *Manna's* in Use are not a *Mel ærium*, or Honey-Dew, as was long believed, but a *Succus proprius* issuing out of some particular Trees, at proper Seasons, and in some Climates only; and that, during the Summer's Heats, a great Number of Vegetables, in almost all the temperate Countries, afford a Juice somewhat akin to *Manna*, from whence the Bee collects and prepares her Honey. It may not, however, be amiss, nor very foreign to our Subject, to exhibit a short Account, how the *Manna Officinarum* is collected.

In *Calabria* and *Sicily*, in the hottest Part of the Summer-Months, the *Manna* ouzes out of the Leaves, and from the Bark of the Trunk, and larger Branches, of the *Fraxinus*, or *Calabrian Ash*. The *Ornus* likewise affords it, but from the Trunk and larger Branches only, and that chiefly from artificial Apertures; whereas it flows from the *Fraxinus* thro' every little Cranny, and bursts thro' the large Pores spontaneously.

What is got from different Parts of the Tree acquires different Names; the Trunks generally afford

* *Actuar. Method. Medend.* L. V. c. 8.

those large white Pieces to which we give the Name of flaky ; but the finest of all is such as is collected from artificial Incisions, in which little Straws, &c. are purposely placed in such a manner, as that the flowing Juice may concrete upon them, and form those long, white, cylindrical, perforated Pieces, which are so much valued.

This Juice is secreted in the largest Quantity, betwixt Noon and Evening. In the Night it is condens'd, if the Season is dry, otherwise the *Manna* is spoil'd : They scrape off the small with wooden Knives, early in the Morning, and gather the larger Flakes ; both which are afterwards dried upon clean Paper in the Sun, till they stick no longer to the Fingers ; and the different Sorts are then carefully pack'd up for Use and Exportation.

XV. *Cyanus Foliis radicalibus partim integris, partim pinnatis, Bractea Calycis ovali, Flore sulphureo ; per Albert. Haller, Prof. Anat. & Bot. Gottingens. R. S. Ang. & Suec. S. descriptus.*

Vide TAB. IV.

Read April 26. 1744. **R**ADIX perennis est, quam eruere nolui, ut parcerem plantæ, quæ semen nondum dedisset.

Folia ad terram conferta, varia : virentia omnia, & firmisscula, cum aliqua, sed brevi & sparsa lanugine. Eorum alia simplicia sunt, longo petiolo, elliptica lanceolata



lanceolata bractea, margine levi. Alia hujusmodi, sed oris serratis. Alia præterea semidivisa, in duos inæquales lobos. Alia varie semipinnata & laciniata. Alia penitus pinnata, qualia continuo dicam, sed pinnis latis, & ultima ampla, pene rhomboidea. Alia, & hujusmodi folia, etiam ad caulem sunt, omnino pinnata, nervo longo, firmo, cavo, oris foliosis, pinnarum paribus numerosis, duodenis & ultra, earum primæ minimæ, simplices & ligulatæ. Sequentes similes, sed accedentibus in origine minoribus, similibus tamen appendiculis. Eundo pinnæ augentur latitudine, & longitudine, & primo semitridæ, deinde semipinnatæ adparent. In fine petiolus latefcit, & foliosus fit; & denuo simplicibus ellipticis pinnulis terminatur.

Caulis cubitalis bracteatus, foliis sub origine ramorum, qualia fere ultimo loco dixi.

Flores satis similes Jaceæ vulgaris laciniatæ, sed egregio gummi guttæ colore conspicui inodori.

Capitis florigeri magnitudo, qualis in icone est.

Calyx globosus superne contractus. Squamarum pars viridis ovalis; pars sicca ovato-rhomboidea, sublutea, tota fimbriata. In supremis squamis habet additamenta sicca, tenuiora, serrata.

Corona petalorum steriliorum, reflexorum, bilabiatorum, quadrifidorum;

Fertilium semen pappo coronatum, flosculus longus, incurvus, quinquesfidus, uno segmento profundius secto. Tubus staminum ex flosculo eminet, ex eo vero tuba clavata.

Semen maturum ex ovali compressum, ciliis nigris coronatum.

Cl. *Gerberus* mist cum nomine Jaceæ laciniatæ flore luteo magno, squamis calycum ciliaribus, splendentibus.

dentibus. Locus natalis ex *Russia*, vel ad *Wolgam* certe flumen, ad cujus litora longo itinere plantas inquisivit.

Ex calyce & flore Jaceæ genus est *Vaillantio*, mihi Cyani, Cl. *Linnaeo* *Centrauræ*; quo tamen nomine nimis multas plantas comprehendit vir egregius, quam ut commoda nomina imponere liceat.

XVI. *A Letter from the Reverend Mr. Roger Pickering, V.D.M. to C. Mortimer, M.D. Secr. R. S. concerning the Propagation and Culture of Mushrooms.*

Deptford, April 19. 1744.

Dear Sir,

Read April 6. 1744. **T**HE late Rains having thrown up upon my Mushroom Beds a great Quantity of those Plants, I take the Opportunity to send some additional Observations to those printed in these *Transactions*, N^o 471. p. 593.

After having repeated the Experiments, then made, upon Plants and Seeds of this Year, I find no Reason to alter any thing there mentioned, either as to the *Lamelle* or Chives on the concave Side of the *Umbella*, being the *Siliquæ* or Seed-vessels; or the Seeds falling from thence to a Lodgement wisely prepared for it on the middle of the *Caulis*, and from thence easily sliding to the Earth contiguous to the Mother-Plant; or as to its Propagation by fibrous Runners, or *Stolones*, like Potatoes; all which, I am persuaded, these

these following new Observations sufficiently confirm.

1. Upon Examination of several *Lamellæ*, I not only distinctly observed Seeds, of Size and Colour proportionable to the Maturity of the Plant, lodged therein, but also a filiquaceous Aperture, with a Row of Seeds ready to fall through it; which is a very evident Proof, that each distinct Chive is a *Siliqua* or Seed-vessel.

2. Upon Observation of the Filament situated on the middle of the *Caulis*, upon which, as I before observed, I at first discovered the Seed, I found both its Contexture and Situation evidently demonstrating the End for which the wise Creator placed it there; *viz.* to intercept the Seeds in their Fall to the Ground; whereby the Power which the Wind would otherwise have upon such minute Bodies is lessened, and the Seed, with little or no Dissipation, securely directed near the Stem of its Mother-Plant. For this Filament is indented and pappous, to catch and lodge the Seed as it falls from the *Siliqua*; and is, at first, rigid, and standing horizontal to the *Umbella* or Head, and at right Angles with the *Caulis*; whereby few or no Seeds can fall without being intercepted: But, as the Plant comes nearer to its Decay, this Filament relents, falls down close to the Sides of the *Caulis*; and its several Indentures then making parallel Lines with the Fibres of the Stalk, the Seeds are, through them, conveyed, as through little Dusts or Channels, to the Ground.

'Tis further to be observed, that this Filament is not of so succulent a Contexture as the *Siliqua* or Seed-vessel; so that the Seeds, which would other-

wife rot in the *Siliqua*, are here retained in full Health, till the Period of their falling to the Ground. I have now by me the Filament of a Plant, laid by for Observation ever since *October* the 28th last past, which is near half a Year ago; from which, two Days ago, I took Seeds fair and perfect.

3. Upon Examination of the *Caulis* in several Sections, I find the Mushroom a Plant more perfect than has been thought. It has a perfect *Radix*; a *Caulis* consisting of Fibres, the Interstices of which are filled up by a parenchymous Substance, leading from the *Radix* to the *Umbella* or Head: It has, as has been observed, its *Semen* and *Siliquæ*, and more regular Periods of Vegetation than is supposed. The common Opinion of a Mushroom's springing up in a Night, and perishing in a Day, has no Foundation in Fact. I have now by me some in all States of Maturity; some of which, to my Knowledge, are near a Fortnight old, and yet but just arrived to a Fitness for the Table.

4. Upon Examination of several Mushrooms, exposed to the open Air, but kept from the Injuries of the Sun and Rain, I find no *Animalcula* bred therein, nor, as yet, a Tendency to Putrefaction; though they have been exposed thus for a Week. On the other hand, upon examining a Mushroom, very far from being full-grown, putrefy'd by the Rain, and Moisture of the Dung in the Bed, I found *Animalcula*, discoverable only by the third Magnifier, floating in the Liquor, squeezed out from it: From which I think it evident, that the dangerous Consequences which History has informed us to have attended the Eating of Mushrooms, have not arose from

from any poisonous Quality essential to them, but from the accidental *Ova* or *Animalcula*, which the Richness of their Nutriment has allured to them, and which their Contiguity to the Ground, and the Places they are produced in, render them obnoxious to. These *Animalcula* I have lately had an accurate View of; but as they demand a fuller Account, than this Paper, already too long, will permit, I shall reserve the Observations upon them for another Opportunity of being honoured with the Attention of the Society.

However, it may not be amiss to subjoin a short Account of the Culture in the Kitchen-garden of a Plant which contributes so much to the Delicacy of polite Tables, which may be depended upon, from personal Trial and Success; as those few Writers upon the Subject, not being acquainted with the true Mushrooms, are not intirely to be depended upon.

In the Melonry, or Place allotted in the Garden for Hot-beds, the Mushrooms must be thus ordered: Having marked out a Portion of Ground one Yard and a half broad, and of any Length, as the Ground will permit; fasten two Sticks at each End of the diametrical Distance already marked out, which shall, by inclining to each other on the Top, form an *Isosceles* Triangle. To the Breadth and Height of these Sticks must the Bed be made, of old, rich, dry Dung, closely trod together: Neither new nor moist Dung is proper; for the Mushroom being naturally of a succulent and spongy Contexture, too much Heat, and too much Moisture, must necessarily injure it.

Having raised your Bed to the Height and Breadth proposed, cover it with fine screen'd Mould, to the Thickness of three Inches, into which, at proper Distances, put either that white fibrous Substance, which may be collected from the Place where Mushrooms have formerly grown; or else water it with Water in which the Chives and Parings of Mushrooms have been steeped; or you may put in the Chives in gross. If you take the first Step, the Mushroom is propagated by Transplantation; that white fibrous Substance, already mentioned, being no other than the *Stolones* of old Mushrooms, from which others are propagated, like Potatoes: If you take the second, that is, by Watering; the Seeds lodged in the Parings being, by the Water, separated from the *Siliqua*, and with it poured upon the Mould, are that which gives Fertility to the Beds thus managed. If you put the Chives in gross into the Mould, it is no more than sowing the Seed in the Pods, as in other Plants it is sometimes necessary to do. Over the Bed, thus prepared, must constantly be kept a Covering of long new Litter, to the Thickness of one Foot, to preserve the Plant from the Frost, the Sun, and the Wind. During the Middle of Summer, and the Extremity of Winter, it is best to make these Beds under Shelter; but at other times they are best exposed, the warm Rains not a little contributing to their Fertility; which, by the sloping Fashion of the Beds, are suffered to moisten them no more than necessary.

I shall only add, that when I speak of the Mushrooms, as I have all along done, I mean the *Fungus*

gus * *porosus*, *crassus*, *magnus*, called, by way of Eminence, in *England*, the *Mushroom*; and shall conclude myself,

S I R,

Your most assured

Friend and Servant,

R. Pickering.

* Mr. *Watson*, a very skilful and ingenious Botanist, was so kind as to remark, that the *Mushroom* here meant, is the *Fungus campestris albus superne*, *inferne rubens*. J. B. See *Raii Synops. Stirp. Brit. Edit. secunda*, p. 11.

ERRATUM.

N^o 461. p. 473. l. 4. from the Bottom, for *Minutes* read *Seconds*.

An Addition to the Reverend Dr. Miles's Account of Fern-Seed, ibid. p. 772. after l. 16.

Whereas I have mention'd, that a sort of *Fungus*, of a light-brown Colour, grows over the Seed-vessels of the *Filix mas*; this is to be understood to have that Appearance, when the Seeds are full-ripe, and the Vessels containing them are prepared to burst: For I have since view'd them, soon after they begin to appear, and also when the Seed-vessels are nearly grown to their full Size; at which times the said *Fungus* is a fine Membrane of a bright Green, intirely covering the Tuft of Seed-vessels like a Cap, and closely adhering to the Surface of the Leaf of the Plant: But when the Seed-vessels are arriv'd at Perfection in Size, and able to bear being expos'd, it begins to recede from the Leaf, and to hang over them in form of an *Umbrella*; and as they grow ripe, it gradually changes brown, and curls up a little, making the Appearance first-mention'd.

Tooting, January 24. 1744-5.

Henry Miles.

ERRATUM.

N^o 472. p. 47. l. 5. read *one with another* in one Case only, where they are supposed to be bent to the same Degree, and that *without shewing*.

PHILOSOPHICAL TRANSACTIONS.

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- I. *A Scheme of a DIARY of the Weather; together with Draughts and Descriptions of Machines subservient thereunto; inscribed to the PRESIDENT and Fellows of the ROYAL SOCIETY; by Roger Pickering, F. R. S. and V. D. M.*

The INTRODUCTION.

Read at a Meeting of the Royal Society, May 3. 1741.

THE Usefulness and Importance of *Meteorological Tables, or Diaries of the Weather*, are too well known to this *learned Society*, to need mentioning with any other View, than as an Excuse under which the Author of the following Observations would shelter himself, for presuming to engage in a Subject, upon which so many, infinitely his Superiors, have written: For, when both the Health and Trade of Mankind considerably depend upon the different States of the *Atmosphere*, the meanest Endeavours to contribute to a Knowledge of it may not be without their Use, and are, at least, excusable.

A Sense of the Importance of observing the Weather induced *Hippocrates*, in his Remarks upon the Epidemic Diseases in *Thasos*, to premise a general History of the Weather preceding them; and with the same View did our great Mr. *Boyle* turn his Thoughts so closely upon the same Subject: whose Example, being followed by several judicious Inquirers into Nature, both abroad and at home, has brought the *Natural History of the Air* to a surprising Degree of Perfection, beyond what the Antients ever

could pretend to, or even thought of. Had but each County in *England* Gentlemen of such Sentiments, who would charge themselves with the annual Trouble of sending a regular Account of the *Weather* to this *learned Body*, by it to be compared and digested, to what Degrees of Accuracy may we not suppose a Knowledge of the Nature and Affections of the *Atmosphere* may be brought; and how well may we not hope to be guarded against the Disorders, which, as *Islanders*, we are exposed to, by such a close Inquiry into the Nature of that necessary Fluid in which we breathe! Not to mention the Advantages which several important Branches of Trade may receive from such Measures: And were the digested Observations of the *Royal Society* compared with those of foreign Societies, formed upon the same Plan, how short a Time would bring this Part of Philosophy to the greatest Degree of demonstrable Certainty!

The Trouble of making and keeping such *Meteorological Registers*, which, in all Probability, prevents several Gentlemen from performing this Piece of Service to the Public, might be rendered very inconsiderable, by the Proposal of an easy, as well as comprehensive, Method for a *Diary*, and a Set of simple and convenient *Machines* for making the necessary Observations.

The Plan of the *Ephemerides Ultra-jestinae*, tho' comprehensive, is, with Submission, very perplex'd; and the several others, mention'd in the *Philosophical Transactions*, perhaps, do not include all the Particulars of which such a *Diary* should consist. The Society of *Edinburgh* has prefaced to their *Medical Essays* a Scheme (which I had not the Pleasure of seeing

seeing till a great while after I had fallen into the following Method) the most calculated for Usefulness; but their *Machines* are neither so simple nor accurate, as such a Work requires; not to mention their being intirely without one for observing the *Force* of the *Wind*.

After these free Expressions, nothing but a sincere Intention would justify me to myself, for presuming to offer the following Plan; except it were a full Dependence upon the Honour and Candour of the *Royal Society*, whose Humanity and Condescension to the meanest of its Well-wishers I have more than once already experienced.

S E C T. I.

An Account of the Diary in general.

ON a Page of a Folio Paper-Book, opening broadways, are drawn, at proper Distances, nine *horizontal*, and seven *perpendicular* Lines; in the void square Spaces of which the Particulars of the *Diary* are written down. The first of the horizontal Lines is for the Days of the *Month* and *Week*, on which the Examination is made: The second for the *Hour* of the *Day*: The third for the *Weight* of the *Air*: The fourth for its *Heat*: The fifth for its *Moisture*, or *Dryness*: The sixth for the *Quarter* of the *Wind*: The seventh for its *Force*: The eighth for the *Weather*; as whether it be *rainy*, or *cloudy*, or *clear*: The ninth for the *Quantity* of *Rain*; and the Space

between the last Line and the End of the Paper, for the *Bill of Mortality*.

The seven *perpendicular* Lines are for the seven *Days of the Week*; which, in our *Diary*, begins with the *first Day*, according to the *Jewish* Account, by us called the *Sabbath*, or *Sunday*. If you therefore carry your Eye along the Paper from Left to Right, you may, at one View, see the *Weight* of the *Air*, and the Degrees of *Heat* and *Moisture*, &c. for the whole *Week*. If you carry your Eye from Top to Bottom down the Column, for any one Day, you see regularly the Whole of the Observations in one Line for that Day. Four Pages, or Weeks, we allow to each Month, and then leave a void Page for the *Observations* made in that Month; and the overplus *calendary* Days are carried on to the Page allotted for the next Month; only taking care to describe in every such Page, where the Ending and Beginning of two different Months are to be found, the Names of both the Months, directly over their final and initial Day.

The Abstract of the *weekly Bill of Mortality* is apparently a Part of Observation peculiar to this Plan, under which Article all *acute Cases*, depending on the *State* of the *Air*, are set down. Perhaps the Ignorance of the *Searchers*, appointed to inspect dead Bodies, as to the precise Diseases People die of, may lay this Method open to Objection: To which it may be sufficient to answer, That this being obviously a requisite Article for a *Diary*, we must be content to take our Advices on this Point from such Hands, rather than none; especially, as all *Political*
Auth.

Arithmetic has always been allowed upon no more certain a Foundation.

S E C T. II.

A View of the Machines in general.

THE *Machines* necessary to the making Observations for a *Diary of the Weather*, are these five :

1. *The Barometer.*

I have found those with *open Cisterns* more sensible than the *portable* ones. That with which I make my Observations, is with an *open Cistern*, furnish'd with a *Micrometer*, that divides an *Inch* into 400 Parts ; by which I am capable of perceiving the most minute Alteration of the *Gravity* of the *Air* : It was made by Mr. *Bird* of the *Strand* ; whose Accuracy in Graduation deserves, I think, Notice and Encouragement.

2. *The Thermometer.*

Mine is one made by *Fahrenheit's* Scale on one Side, with its Correspondence to the Graduation of the *Alcohol* Thermometer on the other.

Of the three next *Machines*, two are *new*, and the other considerably altered, and, I hope, improved, from one offered to the Society a great while ago.

Note, All the *Machines*, except the *Barometer*, are exposed to the open Air. The *Thermometer* and *Hygrometer* are placed in a little *Shed*, made for their Reception, against my *Study-Window*, where I can see the Graduation thro' the Glass; and, by lifting up the Sash, can take them in, as Occasion requires.

S E C T. III.

Of the Hygrometer.

I HAD, for some time, made use of Dr. Hooke's *Hygrometer*, made of the *Beard of a wild Oat*, set in a *small Box*, with a *Dial-plate* and an *Index*; but I soon found an Inconvenience, without the remedying of which no Dependence could be had on this *Machine*; viz. its making more than one *Revolution* in a Night. I endeavoured to remedy this by the following Method, described in TAB I. *Fig. 6.*

At the vertical Point, from which *Moisture* and *Dryness* are graduated, I caused a small *Circle* to be described; the lower *Arch* of which should just intersect with that *Arch*, round which the *Index* of the *Oat* described its Circuit. In the Centre of this small Circle I placed a *Pin*, easily turning in the central Cavity, and furnish'd with a flat Piece of thin *Ivory* on its Head. This Piece of *Ivory*, intersecting with the *Index* of the *Oat*, by it was turned either to the moist or dry Side of its Graduation, as the *Index* made a double *Revolution*. I flatter'd myself with Success; but soon found, in the great Fogs we had last Winter, that the *wild Oat* is not a safe Material to make an accurate *Hygrometer* of: For,

1. In the great Fogs it grew limber; so as that the Weight of the *Index* brought it down upon the Plate, where its Friction prevented its further Motion.

2. It soon loses its *Sensibility*, grows harsh, and is absolutely unfit for Use. So I immediately turned my Thoughts upon some other for my *Diary*, and

reserved this for my Study; where, or in any inclosed Place, it does well enough, and may be very useful in the following Respects; as,

1. To examine, in Cases of Sicknefs, the *Dampness* of *Rooms*.

2. To examine Damps in *subterraneous Cavities*, being let down with a *Weight*, where a Light would sometimes set the Place on Fire.

3. To observe the proper State of Dryness in *Warehouses*, *Wine-vaults*, *Studies*, where Damps would be detrimental and pernicious.

4. To examine the Strength of *sudden Fogs*, and the *comparative* Dampness of particular Situations.

As a *Succedaneum* to this, I thought upon a *statical* one; it recurring to my Mind, That the Weight and Moisture of the Air being but two Properties of one and the same Body, a *statical Hygrometer* (*cæteris paribus*) promised the best Assistance towards a more complete Knowledge of the *Barometer*, which acts upon statical Principles; and that these two Machines must have a reciprocal Correspondence with each other. I then remember'd, that the great Mr. *Boyle* had mentioned something of this Nature; after consulting whom, I made the following *Machine*, acting upon his Principles, but formed in a Manner differing from his.

I caused a *Balance* to be made to turn with *half a Grain*, ordering that the *Axis* of the *Balance* should, on one Side, be drawn out to the Length of one Inch, and its End to be furnished with a *Male Screw*, to which a light *Index* with a *Female Screw* might be fixed. I had this *Balance* fastened in a *Wainscot Box*, twelve Inches in Length, nine in Diameter, and four
in

in Depth at Top, but gradually widening towards the Bottom, with a Back to slide up and down in a Groove. The *Axis*, already mentioned, of an Inch Length, came through a *Hole* in the Front of the Box, and then had the *Index* fastened on, which described the *Segment* of a *Circle* upon a brass Plate, silver'd and graduated into 180 Gr. as if it had consisted of a perfect *Semicircle*, or two *Quadrants*. The Reason why the Graduation did not begin exactly with the diametrical Line was, to prevent the Friction of the *Brachia* of the Balance, with the little Drop placed at the Bottom of the *Axis* already mentioned.

My next Concern was to *charge* this *Balance*. The Beam turned, as has been said, with *half a Grain*; and every such Turn, after repeated Trials, moved the Index somewhat more than one *Degree* of the 180 described upon the *Plate*; so I immediately pitched upon a *Four Penny-weight* all but *six Grains*, which contained as many half *Grains* as there were *Degrees*. This Weight I fixed with a Thread to one *Brachium* of the *Balance*, without any *Scale*, the several Threads or Silk Strings of which, as they would imbibe more Moisture, would make the *Machine* less accurate; and the other *Brachium* I charged with a *Sponge*, suspended likewise by a Thread, of such a Weight, when reduced to absolute Dryness, as made an *Equilibrium*; and then screwing on the *Index* to the first *Degree* of the 180, and exposing the *Machine*, thus ordered, to the open *Air*, in one Night's time the *Index* had got to the 70th *Degree*; which, as the *Sponge* had been absolutely dry, must have been the true *State* of the *Air*, as to *Moisture*, at that time.

I find

I find this *Machine* extremely sensible and accurate; it will alter 10 Degrees in a Night, and as many in a Day; and has, I think, the following Advantages:

1. It is more *portable* than any, except that of the *wild Oat*; and, upon any Accident, more easily and speedily rectified than it, or any other whatever.

2. Being graduated from absolute *Dryness*, it is best calculated for the Discovery of the true State of the *Air*, as to *Moisture*.

3. The near Correspondence between the Degrees on the graduated *Plate*, and the Weight of the *Moisture* necessary to be imbibed or exhaled, to make either *Brachium* of the Balance preponderate every such Degree, gives it the Preference to any other.

For a more perfect Idea of this Machine, see TAB. I. *Fig. 1.* where it is viewed on the Inside, the Back being slid up. At *Fig 2.* is represented the *Plate* with its Graduations and Index, as it should appear on the Front of the Case.

S E C T. IV.

Of the Anemoscope.

THE *Anemoscope* is a Machine four Feet and a Quarter high, consisting of a broad and weighty *Pedestal*, a *Pillar* fastened into it, and an iron *Axis*, of about half an Inch Diameter, fastened into the *Pillar*. Upon this *Axis* turns a wooden *Tube*, at the Top of which is placed a Vane, of the same Materials, 21 Inches long, consisting of a *Quadrant*, graduated and shod with an iron *Rim*, notched to each Degree; and a *Counterpoise*, of Wood as in the Figure, on the other.

Through the Centre of the Quadrant runs an *iron Pin*, upon which are fastened two small round Pieces of Wood, which serve as moveable *Radii* to describe the Degrees upon the Quadrant, and as Handles to a *Velum* or Sail, whose Plane is one Foot square, made of Canvas stretched upon four Battens, and painted. On the upper Batten, next to the shod *Rim* of the Quadrant, is a small *Spring*, which catches at every Notch corresponding to each Degree, as the Wind shall, by pressing against the *Sail*, raise it up; and prevents the falling back of the *Sail*, upon the lessening of the Force of the Wind. At the Bottom of the *wooden Tube* is an *iron Index*, which moves round a circular Piece of Wood fastened to the Top of the *Pillar* on the Pedestal, on which are described the thirty-two Points of the *Compass*.

The Figure of this Machine may be seen TAB. I. Fig. 3. Its Uses are the following :

1. Having a circular Motion round the *iron Axis* and being furnish'd with a *Vane* at Top, and *Index* at the Bottom, when once you have fixed the artificial *Cardinal Points*, described on the round Piece of Wood on the *Pillar*, to the same *Quarters* of the Heavens, it gives a faithful Account of that *Quarter* from which the Wind blows.

2. By having a *Velum* or Sail elevated by the Wind along the *Arch* of the Quadrant, to an Height proportionable to the Power of the Column of Wind pressing against it, the *relative Force* of the Wind, and its *comparative Power*, at any two Times of Examination, may accurately be taken.

3. By having a Spring fitted to the Notches of the *Iron* with which the Quadrant is shod, the *Velum* is pre-

prevented from returning back upon the Fall of the Wind; and the Machine gives the Force of the highest Blast, since the last Time of Examination, without the Trouble of watching it.

I have carefully examined the Dependence that may be had upon this Machine, during the late Storms in *February* 174 $\frac{3}{4}$, by comparing the Height to which the Wind then forced the *Velum*, with the *Deal* Letter. The 19th of *Feb. Sabbath*, 8 a. m. the Anemoscope was at 75: The *Deal* Letter for that Day called it a Storm. The *Saturday* following, being the 25th, at 8 p. m. the Machine was at 79: The *Deal* Letter called that a violent Storm. The *Wednesday* following, the last of *Feb.* it was at 84: The *Deal* Letter called that a violent Storm. So that it appears, that, in such as the Sailors allow to be *violent Storms*, the Machine has hitherto answer'd well, and has had six Degrees to spare for a more violent Gust, before it comes to an horizontal Position.

It is certainly to be depended upon in ordinary Weather, the *Velum* being hung so tender, as to feel the gentlest Breeze. But, after all, I must freely own, that I fear the exposing this Machine to all Winds, for a Continuance, must soon disorder it; and that irregular Blasts and Squalls cannot fail in a short time to impair it. It may not therefore be amiss, to prevent this, for Gentlemen to take the Machine in in violent Weather; and, by taking the *Tube* off the iron *Axis*, to make their Observations with the *Tube*, *Vane*, and *Velum*, in their Hands; which, as it is very light, and far from cumbersome, is easy to do, as I have often experienced.

S E C T. V.

Of the Ombrometer.

THIS Machine consists of a tin *Funnel*, whose Surface is an Inch square, a flat *Board*, and a glass *Tube* let into the Middle of it in a Groove (the Length and Breadth of both Board and Tube being *ad Libitum*), and an *Index*. My Board is about three Feet long, to answer the Height of the Rails that go round the Top of my House, to one of which it is hung, clear of any Obstacle to prevent the free Fall of the Rain, with four little Staples that slide over as many Tenter-hooks. The *Bore* of my Tube is about half an Inch; which, at a Medium, is the best Size, a larger Bore obliging you to make your Graduation the more contracted, and, consequently, the less plain and accurate; and a lesser not permitting you to return the Water out of the Tube when full, without the Adhesion of a great deal to its Sides; which, when you have placed the Tube in its perpendicular Situation, subsides, and sometimes fills up $\frac{2}{3}$ of an Inch; which, without Care, must necessarily make great Mistakes in the *Diary*. The Method of *graduating* the Board is this:

I had a Vessel of Tin made, whose Contents were exactly a *Cubic Inch*. With this Vessel, filled with Water exactly to its Surface, I frequently gauged the Tube, till, by repeated Trials, I had found the Height to which a *Cubic Inch* of Water would rise in it. The Space answering to this on the Board I had graduated into 32 equal Parts, and took the same Method with the rest of the Tube, till in the same manner

ner I had graduated four such Inches. Now the Surface of the Funnel being, as has been said, exactly a square Inch, no Rain can by it get into the Tube, but such as falls within the Square of one Inch; which, as the Shower is more or less, has its exact Quantity shewn upon the Board, on which a moveable *Index* is placed.

This Machine has highly answer'd my Expectation; its Form being very simple, and easily repaired, if any Accident happen. For, should the Tube be broke, 'tis only rubbing out the Graduation, which is marked with a Black lead Pencil upon the Board painted white, and gauging your new Tube with the Cubic Measure for a new Graduation, and your Machine is again complete. I had one Tube broke, and about three Hours Pains set all to-rights. In Winter it will be necessary to let no Depth of Water remain in the Tube; for, should there be a Frost, the Expansion of the Ice will certainly break it. The Machine will equally serve for dissolved Hail and Snow. Its Figure may be seen TAB. I. *Fig. 5.*

S E C T. VI.

Of the Monthly Observations.

THE vacant Page at the End of every four Weeks, reserved for Observations occurring in the preceding Month, and giving a summary Account of the greatest Difference of the Weather in it, is a Method peculiar to this *Diary*; and one which, I flatter myself, will be allowed exceeding pertinent and useful. The great End of this, and all *Diaries*, is to furnish Materials for a Set of sound Observations

tions, upon which to build a thorough Knowledge of the *Atmosphere*, and its Effects upon Mankind : And it is easy to see what great Advantage to this Part of *Natural Knowledge* must arise from a Variety of Observations, made by different Men of Application and Judgment, upon one and the same Subject. Besides, in this Portion of our Design may be included, what could not well without Perplexity be thrown into the Columns of the Diary, all the Meteorological Appearances of the *Aurora Borealis*, Lightning, Thunder, &c. together with Abstracts of the most authentic Accounts of such *Phænomena*, as at any time in the preceding Month have been seen in different Parts of our own Country, or abroad. But this Article must be left to every Gentleman's Judgment ; it opening a fair Field for the most happy Advancements of many Parts of *Natural Knowledge*.

*An EXPLANATION of the Characters in the
DIARY.*

THIS — Line implies the Machine's being at the same Degree as it was in the preceding Observation.

This O Character in the Spaces for the Force of the Wind implies a *Calm*.

Note, 1st, None, but the cardinal and subcardinal *Points* of the *Compass* are commonly marked, unless in case of a *Storm*.

2dly, In the Abstract of the *Bill of Mortality*, which comes out on a *Tuesday*, the Account in each Week is to be compared from the *Tuesday* of the Week before, to the *Tuesday* in that Week where the Abstract is placed.

APRIL 1744.

Days of the Month and Week.	1 Sabbath.	2 Monday.	3 Tuesday.	4 Wednesday.	5 Thursday.	6 Friday.	7 Saturday.
Hours of the Day.	8 a. m. 8 p. m.	8 a. m. 8 p. m.	8 a. m. 8 p. m.	8 a. m. 8 p. m.	8 a. m. 11 p. m.	8 a. m. 8 p. m.	8 a. m. 8 p. m.
Barometer.	196 29— 400	192 29— 400	144 29— 400	246 29— 400	146 29— 400	132 29— 400	392 29— 400
Thermometer.	37	36	37	35 38	35 40	45 40	34 35
Hygrometer.	70	77	79 81	80	74 81	74 77	74
Anemoscope { Quater, W. Force. 28	N. W. 30	N. W. 30	— 74	— 16	S. E. 20	N. W. 16	W. 0
Weather.	Sleet. Rains	Snow. Sleet.	— Cloudy	— Starlight.	Rain. Cloudy.	Overcast. Starlt.	Fine. Overcast.
Orbometer.	13	13 22	3	—	2	1 13	—
Bill of Mortality.	Buried.	Males — 176 Females — 217 Total — 393 Dereased 70	Died of	Apoplexy — 1 Asthma — 8 Colic — 1	Fever — 52 Gripes — 4 Lunatic — 2	Small Pox — 22 Suddenly — 4	—

OBSERVATIONS in APRIL 1744.

Days of the Month. Days of the Week. Hours of the Day.

19	Mond.	M. 12	<p>LAST Night, at 8^r Hour, carefully watched, whether the lunar Eclipse had any Effect upon the Hygrometer; but could not, after several Examinations, perceive that it had any.</p>
20	Friday.	P. M. 8	
24	Tuefd.	P. M. 10 $\frac{1}{2}$	
			<p>This is the first Day of our being favoured with warm Spring Weather. The Thermometer at 8 this Morning was at 65.</p> <p>It now lightens towards the S. E. This is the first we have had this Year.</p>

A SUMMARY of the greatest Difference of the WEATHER in *April* 1744.

Days of the Month.	Days of the Week.	Hours of the Day.			
21	Satur.	A. M. 8	Mercury	Highest being then	30 $\frac{18}{100}$
5	Thursf.	P. M. 11		Lowest - - - -	29 $\frac{11}{100}$
21	Satur.	P. M. 8		Hottest - - - -	75
6	Friday	P. M. 8	Thermometer	Coldest - - - -	34
3	Tuefd.	A. M. 8		Moistest - - - -	81
21	Satur.	P. M. 8		Driest - - - -	65
3	Tuefd.	A. M. 8	Anemoscope	Quarterly most from S. E.	
				Force greatest from N. W.	74
				Weather very unconstant.	
			Ombrometer	Most Rain on 7th in the Night	24 $\frac{2}{32}$
				Total Rain 5 Inch and $\frac{6}{16}$	
				Greatest in the 3d Week	432
			Mortality	Least 1st Week -	393
				Total - - - -	1702

Fig. 1.
p. 17.

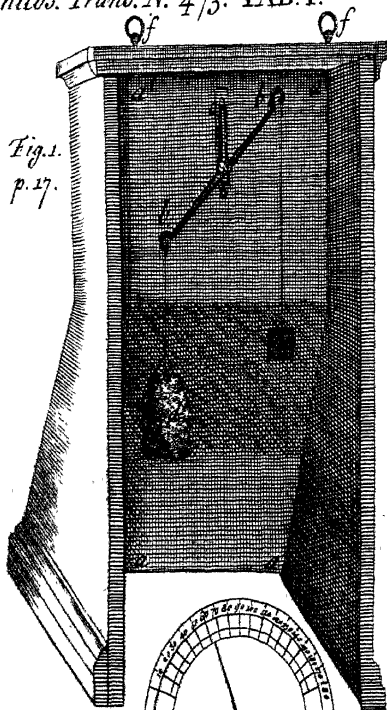


Fig. 2. p. 17.

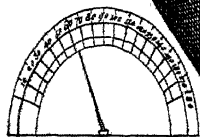
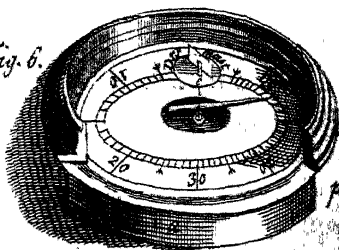


Fig. 6.



p. 18.

Fig. 5. p. 18.

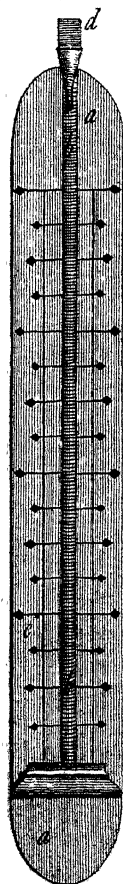
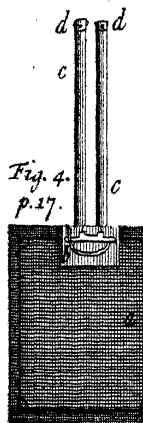


Fig. 4.
p. 17.



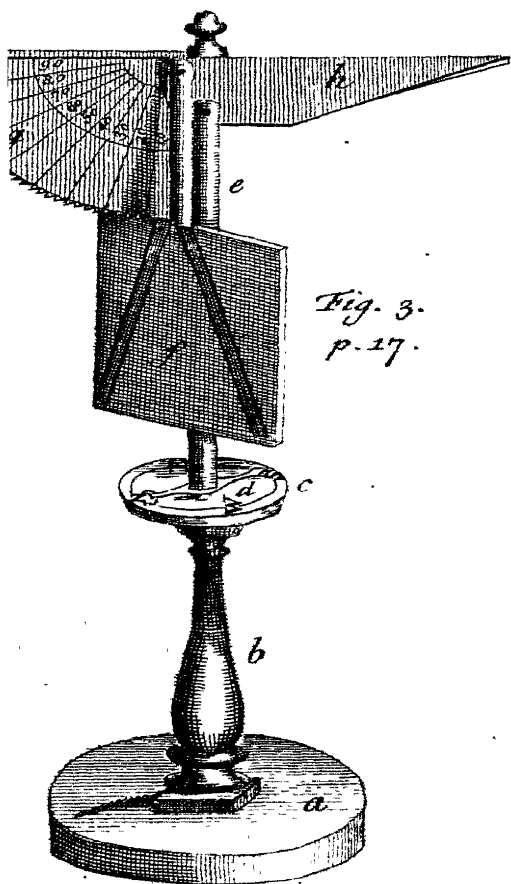


Fig. 3.
p. 17.

A Description of the Figures in TAB. I.

Fig. 1.

- aaaa. The *Hygrometer* seen in the Inside.
- bb. The Balance.
- c. A small Piece of Wood, by which the Balance is fastened to the Box.
- d. The Sponge.
- e. The Weight.
- ff. Two little Rings, by which the *Hygrometer* is hung up.

Fig. 2.

The graduated Plate on the Front of the Machine, with its *Index* and *Divisions*.

Fig. 3. The *Anemoscope*.

- a. The Pedestal.
- b. The Pillar, in which the iron Axis is fitted.
- c. The Circle of Wood, on which are described the 32 Points of the Compass.
- d. The Index.
- e. The wooden Tube upon its Axis.
- f. The *Velum*.
- g. The graduated Quadrant.
- h. The Counterpoise of the Vane.

Fig. 4. The *Velum* taken off.

- a. The Plane of the *Velum*.
- b. The Spring.
- cc. The wooden *Radii*.

C

dd. The

- dd.* The Holes, thro' which the Pin, in the Centre of the Quadrant, goes.

Fig. 5. The Ombrometer.

- aa.* The Board.
bb. The Tube.
c. The Graduation.
d. The Funnel fixed in the Tube.
e. The Funnel one *Inch* square.

Fig. 6. The Wild-Oat Hygrometer.

- a.* The Box and Plate.
b. The wild Oat, with the Index upon it.
c. The Pin, with a small Piece of Ivory on its Head.

II. *A Letter from Mr. William Watſon, Apothecary, F. R. S. to the SOCIETY, concerning ſome Perſons being poiſoned by eating boiled Hemlock.*

Gentlemen,

Read May 10. 1744. **N**otwithstanding the Number of Inſtances, which occur among Writers, concerning the poiſonous Quality of our common Hemlock, or *Cicuta major* of *Caspar Bauhin*; ſuch as, that of *Cardanus* mentioning a Man kill'd by a Cake, wherein this Plant was an Ingredient; that of *Brassavola*, who aſſures us, that it is mortal not to Men only, but to Geefe and Swine; as well as thoſe of

of *Matthiolus*, *Scaliger*, *Kircher*, *Boccone*, and others; yet the Fatality of its Poison, when growing in this Kingdom, has been doubted by many; inas-much as that faithful Collector the late Mr. *Ray* mentions, in his *Synopsis*, *Edit. 2. Pag. 326.* that not only his Friend Mr. *Petiver* eat half an Ounce of the Root of this Plant, but that Mr. *Henly*, a Friend of Mr. *Petiver's*, in his Presence, eat, without any Inconvenience, three or four Ounces of the same Root. From hence it has been thought, either that the Root has Effects different from the Stalks and Leaves, or, that Difference of Climate varies the Degree of the Violence of the Poison.

An Observation indeed of the same kind occurs in the *German Ephemerides*. *Linnaeus*, in the *Hortus Cliffortianus*, makes also some Doubt concerning the Malignity of this Plant; and, in naming it, has kept to the old Appellation of *Theophrastus* and *Dioscorides*, *Conium*; and has transferr'd that of *Cicuta*, to the *Cicuta aquatica* of *Gesner*, and of *Wepfer*. Besides, many of the Accidents, said to have proceeded from *Cicuta* or Hemlock, have been occasion'd by different Plants; some of the Accidents, probably, from the common one, but many more from the *Cicuta aquatica* before-mention'd, and from the *Oenanthe succo viroso*, *Cicutæ facie*, of *Lobel*. This Confusion appears manifestly in several Authors, and some of them of the greatest Credit. Which of these Plants, or whether any of them, was the *Athenian* Poison, nobody has determined.

Altho' the Eating of the Roots, as above-men-tion'd, was attended with no bad Consequences, a late melancholy Accident has been sufficiently con-

vincing of the poisonous Quality of the Leaves of the *Cicuta major*.

On *Sunday, May 6. 1744.* two of the *Dutch* Soldiers lately arrived, who were quarter'd at *Waltham-Abbey* in *Essex*, collected, in the Fields adjoining, a Quantity of Herbs, sufficient for themselves and two others for Dinner, when boiled with Bacon. These Herbs were accordingly dressed, and the poor Men first eat of the Broth with Bread, and afterwards eat the Herbs with the Bacon. In a short time after, they were all seized with violent *Vertigo's*; they soon after were *comatose*; and two of them grew convulsed, and died in about three Hours.

The People of the Town being exceedingly alarmed at this Accident, a Physician (*Dr. Barrowby junior*), being there, immediately went, and order'd the other two, at that time almost dead, large Quantities of Oil; by which means they threw up most of what they had eaten, and afterwards grew better. In all of them the Effects were the same as those from a large Dose of *Opium*.

The next Day, being at the Place, I saw one of these Men much recover'd, and only complaining of a Heaviness in his Head; but the other was so well, as to be gone to perform Exercise with the other Soldiers. There was a fifth Soldier, whom I saw, who told me, He eat some of the Bread out of the Broth, but felt scarce any Inconvenience therefrom. It so happen'd, that the two Men, who gather'd the Herbs, were both killed.

As I went down to the Place to satisfy myself in this Matter, a *Dutch* Officer went with me very courteously to an Inn, where there were two other Soldiers, who had seen and knew the Herbs which
had

had been eaten: He was so kind also as to attend me with these Soldiers into the Fields, to shew me the Plants growing. They first gather'd me the *Cicuta vulgaris* of *John Bauhin*, or Cow-weed; then, the *Myrrhis sylvestris seminibus asperis* of *Caspar Bauhin*, or small Hemlock-Chervil. They then gave me some *Cicuta major*, and, smelling it, immediately said, That this was the Herb that kill'd their Comrades; which I then had no Reason to doubt of, as of the two former Plants: The first grows almost under every Hedge, and is eaten by the Cows, and the other is frequently given to tame Rabbits for Food; whereas Cattle constantly refuse to eat Hemlock.

Before I was thus satisfied, I imagined this Accident to have proceeded rather from *Lobel's Oenanthe*; thinking, that as that Plant grows near the Sides of Rivers, these Soldiers might have gather'd it by the River *Lee*, which runs by the Town, and eaten it for Smallage, to which it has some Resemblance.

It is now known, that the *Cicuta major*, the *Cicuta aquatica*, and the *Oenanthe* of *Lobel*, are certain Poisons; but there are two others of the same Class, growing common in *England*, and not much unlike these in Smell and other Circumstances, vehemently to be suspected: The one is the *Cicuta tenuifolia* of *Mr. Ray*, which grows frequently in waste Places, and in Gardens among Pot-herbs, of which *De la Champ* gives some Account of its Malignancy; the other is the *Cicuta palustris* of *Lobel* and *Tabernaemontanus*, or *Phellandrium* of *Dodonæus*, which grows in muddy Ditches and Ponds.

I don't

I don't remember any History of the pernicious Effects of the *Cicuta major* in this Kingdom; but as the detecting poisonous Plants is of very great Consequence, I presume to lay this Paper before you; and am,

Gentlemen,

London, May 9.

1744.

Your most obedient,

Humble Servant,

W. Watfon.

III. *Methodus Nova Calculi Eclipsium Terræ specialis; vel quorumcunque Occursuum Lunæ cum Stellis, tam errantibus quam inerrantibus: Auctore Christiano Ludovico Gersten, R. S. Sod. & Math. Prof. in Academia Giefensi.*

Presented May 10.
1744.

NEmini, qui limina tantummodo astronomiæ trivit, ignotum quam molesta & plena laboris res sit, calculus Eclipsium Terræ vel quorumcunque appulsuum Lunæ ad stellas. Modus, quibus iste perficitur, quantum ego quidem scio, duplex hucusque extitit. Unus veteribus usitatus, at molestissimus omnium, spectatorem in terram ponit, & ex inventa longitudine & latitudine, prout ex terræ dato loco videntur, Luminarium phænomena solvit. Alter recentior, spectatoris oculum in sole fingit,

fingit, & eclipsium momenta atque phasēs ex projectione quadam circulorum in discum terræ derivat. Posterior, brevior licet atque elegantior, necnon universalitate conspicuus, longam tamen & tædiosam nimis triangulorum analysin requirit, ubi pro speciali quodam terræ loco phænomena investiganda. Movit itaque ipsum laboris tædium, ut de breviori cogitarem. Nec irritò plane successu; nam sub initium anni præterlapsi 1740, calculum hunc novum ad hæc phænomena applicare coepi; & nunc talem in modum perfecisse mihi videor, ut existimem non inutile plane ad communem Astrophilorum usum produxisse inventum, iis præsertim, qui in appulsibus lunæ cum stellis fixis supputandis occupati. Officii igitur & observantiæ causa, sequentes paginas illustris atque celeberrimæ Societatis Regiæ judicio humillime subjicio. Cum vero prolixum nimis foret cuncta demonstrare, fundamenta tantummodo præcipua hujus calculi ad Lemmatum modum præmittam: reliqua ex ipsis, quæ traditurus sum, præceptis in spherica doctrina versatis patebunt. Phænomena spectantem ego cum veteribus in terram pono.

INTRODUCTIO.

SECT. I.

AReus circulorum parallelorum in spherâ gradibus & minutis circuli maximi metiri licet: in calculo præsentis id potissimum requiritur. Extra controversiam positum, circulorum peripherias esse in ratione diametrorum & semidiametrorum. Datur semidiameter circuli maximi, sinus totus; datur & semi-

femidiameter circuli paralleli, cosinus declinationis : inde non difficulter elicitur, quot minuta secunda circuli maximi contineat circuli paralleli gradus unus, determinata ejus declinatione. Nempe ut radius ad numerum minutorum secundorum unius gradus in circulo maximo sic 3600, five cosinus declinationis ad numerum minutorum secundorum in unico gradu circuli paralleli contentorum. Exacto & repetito calculo deprehendimus, arcus unius gradus, circulorum parallelorum, ab uno gradu declinationis usque ad 29 progredientium, æquipollere numeris sequentibus :

Gradus Declin.	Arcus. Circul. Parallel.			Gradus Declin.	Arcus. Circul. Parallel.		
1	59.	59.	27.	16	57.	40.	32.
2	59.	57.	48.	17	57.	22.	41.
3	59.	55.	3.	18	57.	3.	48.
4	59.	51.	13.	19	56.	43.	51.
5	59.	46.	18.	20	56.	22.	53.
6	59.	40.	16.	21	56.	0.	53.
7	59.	33.	9.	22	55.	37.	51.
8	59.	24.	57.	23	55.	13.	49.
9	59.	15.	40.	24	54.	48.	45.
10	59.	5.	18.	25	54.	22.	42.
11	58.	53.	51.	26	53.	55.	39.
12	58.	41.	19.	27	53.	27.	37.
13	58.	27.	43.	28	52.	58.	36.
14	58.	13.	3.	29	52.	28.	37.
15	57.	57.	19.				

Simplici additione ex his, & refectis postea minutis quartis, tabulam condidimus, reductionis arcuum parallelorum ad minuta prima, secunda, &c. circuli maximi, in singulos gradus declinationis ab 1 usque ad 29 ; cujus ope quosvis arcus in circulis parallelis, uno gradu minores, ad minuta prima & secunda circuli maximi revocare licet. Quorum declinatio intermedia, eorum valores quoque ex differentiis ope tabulæ subsidiariæ, non multo negotio inveniuntur.

niuntur. Minuta tertia eum in finem in tabula servavimus, ut quando ultra 50 concreverunt, integrum minutum secundum pro ipsis substitui possit. Exempli gratia sistitur pars tabulæ, circuli nimirum paralleli cujus declinatio 18 gradus.

Arc. Cir. Par.				Arc. Cir. Par.			
Partes circuli max.				Partes circuli max.			
I	II	III	IIII	I	II	III	IIII
II	III	IIII		II	III	IIII	
1	0	57	3	26	24	43	38
2	1	54	7	27	25	40	42
3	2	51	11	28	26	37	46
4	3	58	15	29	27	34	50
5	4	45	19	30	28	31	54
6	5	42	22	31	29	28	57
7	6	39	26	32	30	26	1
8	7	36	30	33	31	23	5
9	8	33	34	34	32	20	9
10	9	30	38	35	33	17	13
11	10	27	41	36	34	14	16
12	11	24	45	37	35	11	20
13	12	21	49	38	36	8	24
14	13	18	53	39	37	5	28
15	14	15	57	40	38	2	32
16	15	13	0	41	38	59	35
17	16	10	4	42	39	56	39
18	17	7	8	43	40	53	43
19	18	4	12	44	41	50	47
20	19	1	16	45	42	47	51
21	19	58	19	46	43	44	54
22	20	55	23	47	44	41	58
23	21	52	27	48	45	39	2
24	22	49	31	49	46	36	6
25	23	46	35	50	47	33	10

Arc. Cir. Par.	Partes circuli max.			Arc. Cir. Par.	Partes circuli max.		
	I	II	III		I	II	III
II	II	III	III	II	II	III	III
51	48	30	13	56	53	15	32
52	42	27	17	57	54	12	36
53	50	24	21	58	55	9	40
54	51	21	25	59	56	6	44
55	52	18	29	60	57	3	48

Exemplum.

Sint $53^{\circ} 47''$ hujus circuli paralleli convertenda in partes circuli
maximi: fiat $53^{\circ} = 50^{\circ} 24'' 21'''$
 $45'' = 42 47$

Summa $54^{\circ} 7''$ erit valor quæsitus.

SECT. II.

Circulorum ad æquatorem parallelorum portiones
exiguas, ubi pro rectis tuto assumi possunt, se-
cantur a circulis declinationum ad angulos rectos.
Quapropter triangulum sphericum parvum, cuius latus
unum portio circuli declinationis, alterum portio cir-
culi paralleli, pro triangulo plano rectangulo haberi,
& ejus hypotenusa per theorema Pythagoricum vel
alias regulas trigonometriæ planæ tuto eruitur. Cum
vero hæc hypotenusa sit diagonalis quadrilinei cuius-
dam spherici, quod sectione duorum circulorum de-
clinationis, per duos ad æquatorem parallelos effe-
ctum, ex arcubus parallelis major, & a polo remotior,
pro basi trianguli rectanguli eligendus, ubi de hypo-
pothenusa invenienda quæritur.

SECT.

S E C T. III.

TAbula parallaxium altitudinis lunæ duplici modo construuntur. Primum secundum præcept. XII. *Streete*, tabulis *Carolinis* præmissum, deinde secundum præcept. XIII. ejusdem. Pro distantia lunæ a terra, sufficit ratio hujus distantiae ad semidiametrum terræ, quæ ex parallaxi horizontali statim innotescit. Prior modus parallaxes determinat ad altitudines visas; sc. supra horizontem sensibilem. Pro eclipsibus terræ, & appulsubus lunæ ad stellas, prior modus est eligendus, non posterior. Secus qui ageret, in calculum nostrum errores non contemnendos intruderet. Accuratam parallaxium altitudinis tabulam, cum rem maximi momenti esse deprehenderem, de novo ad usus meos usque ad 70 gr. altitudinem construxi, cum qua tamen postea satis bene consentire deprehendi *Lansbergianam in tab. motuum cælestium hujus authoris*, p. 48. & seq. Quæ vero in *Ludovicianis* extat N^o XXV. ea ad altitudines visas, non veras, respicit, adeoque absque reductione ad hos usus minus idonea. Notandæ velim parallaxes ejusdem altitudinis veræ, sed diversarum distantiarum lunæ a terra esse ipsis distantiiis per consequens parallaxibus horizontalibus proportionales.

Sequens abacus exhibet parallaxes altitudinis ex nostra & *Lansbergii* tabula, qui numeri, in ratione aliarum parallaxium horizontalium aucti vel diminuti, vel soli ad quoscunque casus sufficiunt.

Alt. veræ.	Parall. Ex Tab.	Alt. noſtr.	Parall. Lansberg.	Alt. veræ.	Parall. Ex Tab.	Alt. noſtr.	Parall. Lansberg.		
1	60	0	59	59	36	49	3	49	4
2	59	59	59	59	37	48	26	48	27
3	59	58	59	57	38	47	48	47	49
4	59	56	59	54	39	47	9	47	10
5	59	52	59	50	40	46	30	46	31
6	59	47	59	46	41	45	49	45	51
7	59	41	59	40	42	45	7	45	9
8	59	34	59	33	43	44	25	44	26
9	59	26	59	24	44	43	42	43	42
10	59	17	59	14	45	42	58	42	58
11	59	6	59	4	46	42	13	42	13
12	58	55	58	53	47	41	28	41	27
13	58	42	58	41	48	40	41	40	37
14	58	28	58	28	49	39	54	39	54
15	58	14	58	14	50	39	6	9	7
16	57	58	57	58	51	38	17	38	18
17	57	41	57	41	52	37	28	37	28
18	57	23	57	23	53	36	38	36	37
19	57	4	57	3	54	35	47	35	46
20	56	44	56	43	55	34	55	34	55
21	56	23	56	22	56	34	3	34	3
22	56	0	56	0	57	33	10	33	10
23	55	37	55	36	58	32	17	32	16
24	55	12	55	11	59	31	23	31	22
25	54	47	54	46	60	30	28	30	28
26	54	21	54	20	61	29	33	29	33
27	53	54	53	53	62	28	37	28	37
28	53	25	53	25	63	27	41	27	41
29	52	56	52	56	64	26	44	26	44
30	52	26	52	25	65	25	47	25	47
31	51	54	51	53	66	24	49	24	49
32	51	22	51	21	67	23	50	23	50
33	50	48	50	48	68	22	51	22	51
34	50	14	50	14	69	21	52	21	52
35	49	39	49	40	70	20	52	20	52

S E C T. IV.

Data longitudine & latitudine ſideris, datur, per regulas trigonometricas, ejus aſcenſio recta & declinatio. Sed moleſtam id triangulorum analyſin requirit:

præstat tabulis hunc in finem conditis uti. Habemus in *Historia cælesti Flamstedii* duplices *Abrahami Sharpii*; quibus non modo ex ascensione recta & declinatione fit conversio in longitudinem & latitudinem, sed & ex longitudine & latitudine in ascensionem rectam & declinationem. Quæ posteriores sunt ordine pag. 34 & 74 *Tom. III.* viam ducunt omnium brevissimam; propterea hucusque in calculo nostro his usi sumus. Cui apparatus harum tabularum sumptuosior videatur, sciat, lunam ultra 5 latitudinis gradus non multum vagari; perpaucae igitur paginae ex eis pro calculo nostro sufficiunt. Siquis eas legitimo modo interpolando, vel tabulas subsidiarias construendo, prolixiores reddere velit, is compendium sibi & commodum non contemnendum parabit. Breviter his præmissis, propero nunc ad

CALCULI PRÆCEPTA.

1. Posteaquam per modos usitatos cognitum eclipticam terræ in copula solis & lunæ futuram esse, ex tabulis theoricis inveniatur tempus conjunctionis, longitudo & latitudo lunæ, motus ejusdem horarius verus, parallaxis, atque diameter horizontalis, necnon motus horarius solis, ejusdemque diameter.

2. Ope tabularum, ex datis longitudine & latitudine, definiantur ascensiones rectæ solis & lunæ, & declinationes.

3. Tempore medio in apprensus converso, si conjunctionis momentum accidit ante meridiem, hora una ante illud, per motum horarium, ad eclipticam reductum, determinentur longitudines solis & lunæ, latitudo lunæ, & singulorum punctorum quarantur
ascensiones

ascensiones rectæ & declinationes. Si post meridiem fit copula, idem faciendum hora una post conjunctionem.

4. Tempus conjunctionis, nequa hoc ipsum hora 1 diminitum subtrahatur a 24 horis, quando id accidit, ut habeatur intervallum temporis a conjunctionis momento, vel ab hora 1 ante conjunctionem, usque ad meridiem. In horis pomeridianis ipsum tempus dat intervallum.

5. Inventa intervalla temporis convertantur in gradus & minuta æquatoris; & prodeant sic anguli circuli declinationis per centrum solis transeuntes cum meridiano loci.

6. Ascensio recta lunæ vel major vel minor esse potest ascensione recta solis quocunque tempore. Horis matutinis, si minor ea est, tunc differentia inter ascensiones rectas solis & lunæ subtrahenda est ab angulo circuli declinationis numero præcedente invento; si major, addenda ad eundem angulum, & habetur angulus circuli declinationis per centrum lunæ transeuntis cum meridiano loci. Contrarium faciendum horis pomeridianis.

7. Ex inventis (numero præced.) angulis, declinationibus solis & lunæ, (num. 2.) & latitudine loci, per trigonometriæ sphericæ regulas, supputentur altitudines veræ solis & lunæ in utroque casu: deinde &

8. Anguli circulorum declinationis, per centrum lunæ in utroque casu transeuntium cum circulis verticalibus. Minuta secunda in hoc & præcedente numero suo negliguntur.

9. Inventis altitudinibus veris lunæ (num. 7.) ipsius parallaxi horizontali, (num. 1.) per tabulas parallaxium

rallaxium altitud. reperiuntur parallaxes altitudinis lunæ. Uti Soli parallaxis horizontalis cum *Flamstedio* 10 secundorum tribuenda censetur, parallaxis lunæ horizontalis hac quantitate prius minuenda.

10. Fiat, ut radius ad numerum minutorum secundorum in parallaxi altitudinis (num. præced.) inventæ contentorum; sic sinus anguli (num. 8.) inventi ad quartum proportionalem numerum, quem edit calculus, voco *parallaxin ascensionis rectæ in circulo parallelo*.

11. Pergatur, ut radius ad eundem numerum minutorum secundorum in parallaxi altitudinis comprehensorum; sic co-sinus anguli (num. 8.) inventi ad quartum proportionalem, qui *parallaxis est declinationis lunæ*. In utroque casu, momento nempe conjunctionis, & hora ante vel post conjunctionem, hic calculus instituendus.

12. Disponantur ascensiones rectæ solis & lunæ in ambobus casibus secundum ordinem naturalem numerorum. Differentia inter ascensiones rectas solis addatur ad primam ascensionem rectam lunæ, eliminetur prima ascensio recta solis, remanebunt tunc duæ ascensiones rectæ lunæ & una solis.

13. Declinationes solis aut crescunt aucto tempore, aut decrescunt. Priori casu, differentia earum addatur ad eam declinationem lunæ, quæ minimæ ascensioni rectæ competit. Priori casu subtrahatur, eritque mutua distantia luminarium, quasi sol immotus per totum horæ spatium lunam progredientem respiceret.

14. Singulæ ascensiones rectæ subtrahantur, minor quælibet a maxima, & probe notentur differentiæ.

15. Parallaxes declinationis subtrahantur a declinationibus lunæ, si hæc quidem sunt boreales; at vero si australes existunt, addantur. Sic prodeunt declinationes lunæ visæ.

16. Differentiæ num. 14. inventæ, quæ nunc in circulo parallelo esse concipiuntur, ope tabulæ reductionis, supra § 1. *Introduc.* alleg. reducantur, ad minuta prima & secunda circuli maximi. Paralleli declinatio eadem, quæ minima declinatio visæ Lunæ aut Solis. A numero & distantia punctorum ascensionis rectæ, a principio arietis nunc penitus abstrahendum: non enim id agitur, sed tantummodo de positione & distantia luminarium inter sese solliciti sumus.

17. Si ante meridiem incedit luna, tunc parallaxes ascensionis rectæ in circulo parallelo num. 10. repertæ addantur competentibus lunæ locis. Sin vero post meridiem id accidit, loco additionis fit subtractio. Hoc demum peracto, determinatæ sunt positiones & loca visæ luminarium, tempore conjunctionis veræ, & hora 1. ante vel post eandem, quibus deinde facili negotio, quæ restant elicienda. Nam,

18. In omni casu ex repertis fit triangulum rectangulum, cujus Basis distantia locorum apparentium lunæ in circulo parallelo; Cathetus differentia declinationum visarum ejusdem; Hypothenusa dat orbitam visam; & positio solis, siue intra siue extra triangulum cadat, satis quoque erit determinata. Ipsum triangulum nunquam ad eam magnitudinem assurgit, quæ obster quo minus pro plano & rectilineo sumi queat. Hinc simplicissima & facili constructione ope circini & scalæ determinari possunt distantia centrorum minima & puncta in orbita, ubi accidunt initium eclipsis, maxima obscuratio

obscuratio & finis adeo exacte, si scala idonea adhibeatur, ut ne 1 vel 2 minuta secunda quidem deficiant; vel, si mavis, hæc, & reliqua omnia per trigonometriæ planæ regulas perficiuntur.

19. Quando summa semidiametrorum apparentium solis & lunæ extra fines hypotenusæ hujus trianguli cadit, tunc hæc quidem continuanda, donec occurrat; & reliqua usitato more peragenda, ut habeatur tempus initii & finis eclipsis. Sed tunc, ubi puncta occurfus longe nimis a trianguli punctis jam determinatis distant, calculus erit corrigendus, si exacte tempus initii & finis quaeritur. Etenim supponitur semita lunæ apparens in linea recta, & motus visus æquabilis; ex quibus neutrum verum est, utut via visâ unius horæ intervallo, ita parum plerumque in eclipsibus a rectitudine divergat, ut absque errore conspicuo pro recta linea assumi possit. Non item tamen de celeritatis æqualitate dicendum. Correctionis ergo calculus instituendus, quem exemplo potius mox sequenti, quam regulis, docebo.

Hæc quidem sunt methodi nostræ præcepta præcipua: quæ restant, exemplum illustrabit. Me non movente videbunt intelligentes, eam tam ad occurfus lunæ cum reliquis planetis tam ad appulsus ad inerrantes stellas facile applicari posse. De præstantia & differentia ab aliis hucusque receptis nolo verba facere: penes alios id judicium esto. Nunc id ago, ut eam ad usus meos multo breviorē facilioremque reddam. In tuto re est, scio, sed nondum labor finitus. Nempe pro altitudine poli *Giecenfis*, quilibet gradus declinationis habet, in quolibet temporis momento, determinatam altitudinem veram, & determinatum angulum circuli declinationis cum meri-

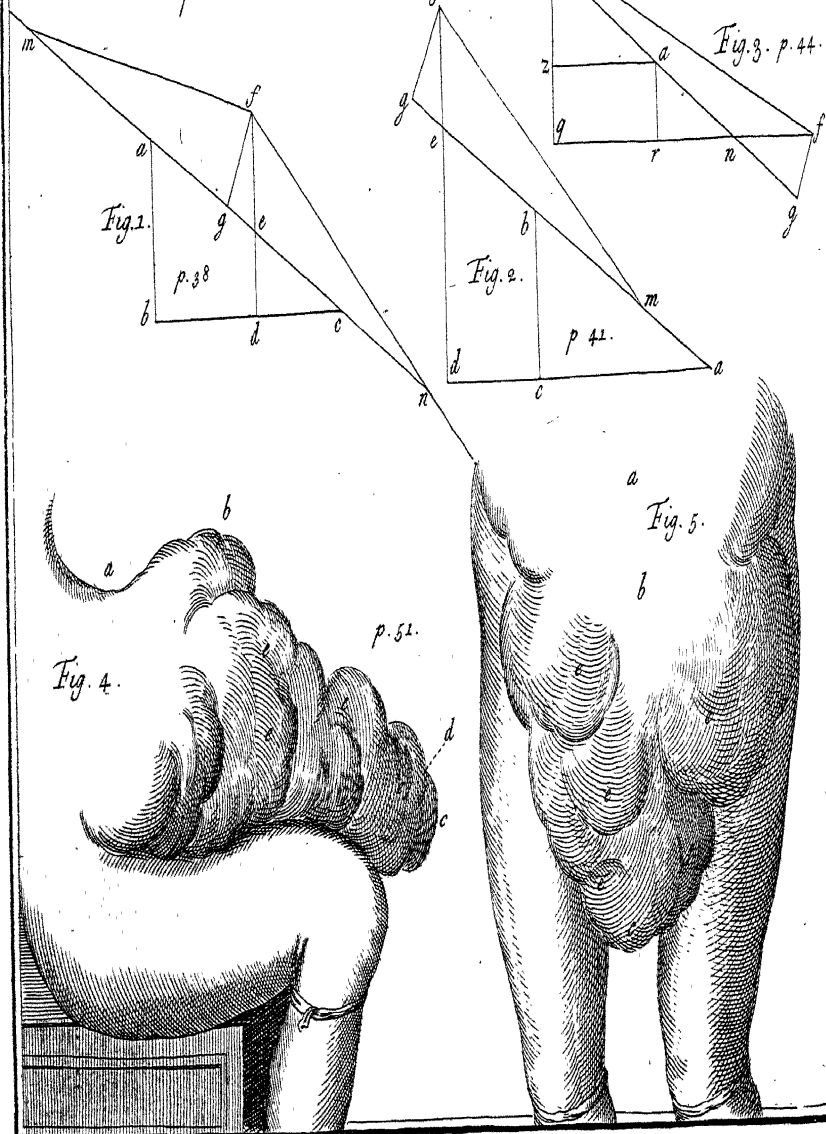
diano loci. Ab his dependent parallaxes declinationis & parallaxes ascensionis rectæ in circulo parallelo. Tabulam igitur molior, ad quosvis gradus declinationis lunæ & in singula quatuor minuta prima temporis mihi reddituram tum parallaxin declinationis, tum parallaxin ascensionis rectæ in circulo parallelo. Parallaxium basin statuo, horizontalem unius gradus : sed parallaxes ejusdem altitudinis sunt in ratione directa parallaxium horizontalium, ut supra § 3. *introduc.* monui ; per consequens, in eadem ratione sunt parallaxes declinationis, & parallaxes ascensionis rectæ, in circulis parallelis : ergo pro latitudine hujus loci unica hæc tabula sufficiet, adhibita alia subsidiaria, cujus ope parallaxes ad quamvis aliam basin reducuntur. Parallaxes ascensionis rectæ deprehendi propemodum esse constantes in quibusvis declinationis gradibus ; ergo cum his, leve negotium, gravius & operosius erit cum parallaxibus declinationis. Sed de his fortasse alibi ; pergamus nunc ad

EXEMPLUM.

Anno Christi 1706, *Maii* die 12, accidit eclipsis terræ. Quæritur ad longitudinem & latitudinem observatorii *Parisiensis*, ejus quantitas, initium, maxima obscuratio, & finis. Secundum tabulas *Ludovicianas* accidit conjunctio solis & lunæ die *Maii* 11, hor. 21, min. 49, sec. 13, secundum tempus medium. Ad hoc tempus secundum easdem tabulas

11

1. Locus verus ☉ & ☾ in ecliptica	- -	51	6	48
Longit. ☾ in orbita	— —	51	8	22
Locus ☾	— —	44	14	59
Argumentum				



			°	'	"
Argumentum latitudinis	—	—	6	53	23
Latitudo ☾ borealis	—	—	36	7	
Motus horarius ☉	—	—	2	25	
Semidiameter ☉	—	—	15	54	
Motus horarius ☾	—	—	37	13	
Motus horarius ☾ ad eclipticam reduct.			37	5	
Semidiameter ☾ horizontalis	—	—	16	31	
Parallaxis ☾ horizontalis	—	—	60	29	

Secundum Tab. Abrahami Sharpii.

Ascensio recta ☉	—	—	48	37	57
Declinatio ☉ boreal.	—	—	18	3	32
Ascensio recta ☾	—	—	47	53	27
Declinatio ☾ boreal.	—	—	18	25	58

Æquatio temporis sec. tab. *Ludovicianas* est 8' 18".
Addendum ad medium, ut fiat apparens. Ergo tempus verum conjunctionis est h. 21, 57' 31".

2. Ad horam 1. ante conjunct. longitudo ☉ = 51° 4' 23". Longitudo ☾ = 50° 29' 43". Latit. ☾ boreal = 32' 53". per consequens incrementum latitudinis unius horæ intervallo = 3' 15". Ascensio recta ☉ per tab. *Abrahami Sharpii* = 48° 40' 24". Declinatio ☉ = 18° 4' 10". Ascensio recta ☾ = 48° 30' 21". Declinatio ☾ = 18° 38' 59".

3. Intervallum a momento conjunctionis, sc. 21^h 57' 31", usque ad meridiem, est = 2^h 2' 29"; quod in arcus æquatoris conversum = 30° 37' 15". Ab hora 1 ante ☉ usque ad meridiem præterlabuntur 3^h 2' 29"; quibus respondet arcus æquatoris, 45° 37'

15". Adfunt igitur ad normam præcept. 5. anguli circulum declinationis per centrum \odot tranfeuntium, cum meridiano loci in utroque cafu.

4. Afcenfio recta \odot præcedit afcenfionem rectam \mathbb{C} in duobus hīs cafibus: ergo, per præcept. 6, differentia ab his repertis angulis fubtrahendæ; fc. in δ differentia afc. rect. \mathbb{C} ab afc. rect. \odot eft $10' 3''$. Hora 1 ante δ vero eadem differentia = $43' 38''$. Ergo fubductis hīs arcubus, manet pro angulo circuli declinationis per centrum \mathbb{C} tranfeuntis in δ $30^{\circ} 27' 12''$, hora 1 ante δ , $44^{\circ} 53' 37''$.

5. Hifce angulis, elevatione poli obfervatorii *Parifienfis* = $48^{\circ} 50'$, & declinationibus \mathbb{C} , confequuntur altitudines \mathbb{C} . Speciatim in conjunctione altit. \mathbb{C} = $51^{\circ} 5'$, hora 1 ante δ alt. \mathbb{C} = $42^{\circ} 52'$. Necnon anguli circulum declinationis cum verticalibus ad conjunct. prodit $32^{\circ} 4'$ ad horam 1 ante δ $39^{\circ} 19'$.

6. Secundum tabulam, noſtram 1, vel partem § 3. *introductionis* exhibitam, ad parall. horizontalem $60' 29''$, parallaxis altitudinis \mathbb{C} in δ = $38' 31''$; non fubtracta parallaxi \odot ab horizontali, quod hoc exemplo confulto omifimus. Parallaxis afc. rect. in circulo parallelo = $20' 27''$. Parallaxis declinationis deprehenditur = $32' 38''$, per præcept. 10 & 11. Sed, ad horam 1 ante δ , parallaxis altitudinis = $44' 53''$, parall. afc. rect. in circ. parallel. = $28' 26''$, parallaxis declin. = $34' 43''$.

7. Sequitur nunc, per præcept. 12, difpofitio & fubtractio afcenf. rectarum, & declinationum afc. rectis competentium.

	Asc. rect.			Delin. Comp.			
	°	'	"	°	'	"	
Ad hor. 1 ante ☉.	☾	47	53	35	18	26	0
Ad ipsam ☉	☾	48	30	21	18	38	59
Ad hor. 1 ante ☉.	☉	48	37	57	18	3	32
Ad ipsam ☉.	☉	48	40	24	18	4	10
Diff. inter asc. rect.	☉		2	27	Inter declin.	☉	38

	Asc. rect.			Declinat.			
	°	'	"	°	'	"	
Ad hor. 1 ante ☿	☾	47	56	2	18	26	38
In ipsa ☿ ☾		48	30	21	18	38	59
Immoti ☉		48	40	24	18	4	10
Diff. <i>a</i>		34	19				
Diff. <i>b</i>		44	22				
				Parall.	{	34	43
				declin.		32	38

$$\text{Declin. visæ, } \left\{ \begin{array}{l} \text{☾ } 17 \quad 51 \quad 55 \\ \text{☉ } 18 \quad 6 \quad 21 \\ \text{☉ } 18 \quad 4 \quad 10 \end{array} \right.$$

8. Secundum præcept. 16. differentia *a* reducta ad partes circuli maximi = $32' 39''$; differentia *b* = $42' 13''$. Prior est distantia locorum lunæ in utroque casu, posterior distantia solis immoti, a loco primo lunæ in circulo parallelo, cujus declinatio $17^\circ 51' 55''$; vel, quod parum differt, $17^\circ 52'$.

9. Parallaxis asc. rect. in cir. parallelo in ☉ = $20' 27''$, (num. 6.) addita, per præcept. 17. ad locum lunæ secundum, $32' 39''$ efficit $53' 6''$. Locus ergo primus ☾ = parallaxi asc. rect. ad hor. 1 ante ☉. Hinc in circulo parallelo sunt loca visæ luminarium sequentia:

Ad hor. 1 ante \odot	\odot	28	26 = A
\odot immoti		42	13 = B
In ipsa \odot	\odot	53	6 = C
<hr/>			
Diff. inter A & B		= 13	47
Diff. inter A & C		= 24	40

A declinationibus visis si subtrahitur minima declinatio, hoc casu \odot $17^{\circ} 51' 55''$ manet pro \odot $12' 15''$; pro \odot in \odot $14' 26''$.

10. Esto nunc bc (fig. 1. TAB. II.) portio circuli paralleli ad declinationem $17^{\circ} 51' 55''$; & in eo punctum c , centrum \odot ad hor. 1 ante \odot , d locus \odot , b locus \odot in \odot , erit $dc = 13' 47''$; $bc = 24' 40''$. Ex punctis d & b erigantur perpendiculares df & ab ; quarum prior = $12' 15''$, minimæ sc. diff. declinat.; posterior = $14' 26''$, maximæ, erit f centrum solis immoti, a centrum lunæ in ipsa \odot . recta ac , semita visa lunæ unius horæ intervallo.

11. A puncto f ad ac , demissa perpendicularis, gf quantitatem eclipsis, punctum g obscurationem maximam determinat. Quod si, porro, circino capiatur intervallum, nf & fm = summæ semidiametrorum apparentium \odot & \odot , eoque ex puncto f secetur hypotenusa producta mn , trianguli abc , efficietur determinatio punctorum n & m , in quibus accidit initium & finis eclipsis.

12. Per calculum trigonometricum prodit $cg = 18' 4''$; $gf = 3' 37''$; $ac = 28' 34''$. Si inferatur ut ac ad g c , sic tempus per ac = 1 hor. ad tempus per gc , resultat $37' 57''$; hoc tempus additum ad h. 20, $57' 31''$, (1 hor. sc. ante \odot) efficit momentum maximæ obscurationis, h. 21, $35' 26''$.

13. Semidiameter ☉ horizontalis est = $16' 31''$ (num. 1.); sed per tabulam *Hireanam* xxiv., correcta = $16' 43''$. Semidiameter ☉ = $15'' 54'$. Summa semidiametrorum ☉ & ☾ = $32' 37''$: subducta gf ab hac summa, restat pars deficiens, = $29' 0''$, hæc in digitos eclipticos redacta, dat quantitatem eclipsis 10 digit. 56 min.

14. Ad initium & finem determinandum, ex $g f$, $f n$, & $f m$, quærenda est $g n$ & $g m$. $f n$ æqualem facio summæ semidiametrorum apparentium, (num. præced.) uno vel duobus minutis secundis deminutæ, $f m$ vero = eadem summæ, sed uno vel duobus minutis secundis auctæ; adeoque $f n = 32' 35''$; $f m = 32' 39''$. Quamobrem $g n = 32' 22''$; $g m = 32' 25''$; tempus per $g n =$ h. 1. $7' 58''$; quod, subtractum a momento obscuracionis maximæ, exhibet initium eclipsis, sc. h. 20, $27' 28''$: tempus per $g m =$ h. 1, $8' 5''$; quod, additum ad obsc. max. dat finem h. 22, $43' 31''$.

Correctio Initii.

15. Hor. 1 ante ☉ = hor. 20, $57' 31''$; tempus initii = 20 h. $27' 28''$; initium ergo distat ab hor. 1 ante ☉ $30' 3''$. Huic diff. temporis competit motus ☾ in longit. $18' 34''$; incrementum latit. ☉ $1' 37''$; motus ☉ in longit. $1' 12''$: his subductis a longitudinibus & latitudine ad hor. 1 ante ☉, relinquitur ad tempus initii, longitudo ☉ = $51^{\circ} 3' 11''$; longitudo ☾, $50^{\circ} 11' 9''$; latitudo ☾, $31' 16''$; asc. rect. ☉ $48^{\circ} 36' 44''$; declinat. ☉ $18^{\circ} 3' 13''$; asc. rect. ☾ $47^{\circ} 35' 10''$; declinat. ☾ $18^{\circ} 19' 28''$: differentia inter asc. rect. ☉ & ☾ = $1^{\circ} 1' 34''$: intervallum temporis a momento initii usque ad meridiem, =

I

hor.

[40]

hor. 3, 32' 32"; quod, in arcus æquatoris conversum, dat 53° 8' 0". Nunc, quoniam asc. rect. ☿ minor asc. rect. ☉, differentia ascensionum rectarum ☉ & ☿ subtrahenda ab hoc arcu, remanet 52° 6' 26", angulus sc. circuli declinationis per centrum ☿ transeuntis cum meridiano loci. Altitudo ☿ = 38° 20' ang. circ. declinationis cum verticali = 41° 28'. Parallaxis altit. = 47' 58". Parallaxis asc. rect. in circ. parallelo = 31' 45". Parallaxis declinationis = 35' 56".

16. Dispositio & reductio ascensionum rectarum, secundum præcept. 12. nunc talis :

Asc. rect.				Declin. Comp.			
Q / "				0 / "			
Ad hor. 20, 27' 28" ☿	47	35	10	—	—	18	19 28
Ad hor. 1 ante ☿	☿	47	53 35	—	—	18	26 0
Ad hor. 20, 27' 28" ☉	☉	48	36 44	—	—	18	3 13
Ad hor. 1 ante ☉	☉	48	37 57	—	—	18	3 32
Diff. asc. rect. ☉				Diff. declin. ☉			
☿				☿			
☿				☿			
Immoti ☉				Immoti ☉			
Different. a				Parall. declin.			
Different. b				{			
Diff. a reduct.				Declin. visæ			
Diff. b reduct.				{			
Parall. { ad h. 1 a. ☿				Diff. c			
asc. rect. { ad h. 20, 27' 28"				Diff. d			
☿							
☿							
☉							
Diff. e							
Diff. f							

Fig. 2.

17. Ex differentiis e, f, c, d , construitur typus & correctio sequentem in modum. Diff. $e = 13' 5''$ fit $= ac$ (Fig. 2.); diff. $f = 26' 54''$, fit $= ad$: perpendicularis bc , fit $=$ diff. c sive $7' 26''$: perpendicularis fd fit $= 19' 41'' =$ diff. d ; eritque h. 20, 27' 28'', centrum C in a ; hor. 1 ante δ vero in b ; centrum \odot immoti in f . Orbita lunæ visa, determinatur per puncta a & b ; quoniam per ea transit. Quod si fm sit æqualis summæ diametrorum apparentium $= 32' 35''$, hæc ab hypothenuſa ba , partem ma , reſecat, quæ in tempus converſa dat correctionis quantitatem.

18. Si calculo res peragenda, ba continuanda, & ex f perpendicularum fg in eam demittendum. In caſu præſenti eſt $ab = 15' 2''$, $ae = 30' 55''$, $ge = 2' 10''$: ergo $ga = 33' 5''$, $gf = 3' 50''$, $fm = 32' 35''$; ergo $gm = 32' 21''$; & $ga - gm = ma = 44''$; quæ quantitas, in tempus converſa $= 1' 27''$. Cum autem C moveatur ab a verſus b , & in a poſitum ſit centrum lunæ hor. 20, 27' 28'', manifeſtum eſt hoc tempus addendum eſſe ad tempus initii ſupra inventi, ut fiat verum & correctum initium eclipſis; ſc. h. 20, 28' 55''.

Probatio Correſtionis.

19. Exactitudinem calculi ut oſtendam, inveſtigemus diſtantiæ centrorum \odot & C ad hoc tempus initii correcti. Nam ſi hæc ſummæ ſemidiametrorum apparentium æquales; verum neceſſario eſt momentum initii; ſi ſecus, falſum eſt. Tempus quod præterlabitur ab hoc momento initii correcti, ad tempus $\delta =$ h. 1, 48' 36''. Huic competit motus C in ecliptica $54' 46''$;

F

increment.

increment. latit. \odot $4' 48''$; morus \odot in longitudine $3' 34''$: ergo tempore initii correcti longit. \odot $50^{\circ} 12' 12''$; latit. \odot hor. $31' 19''$; longit. \odot $51^{\circ} 3' 14''$; ascens. rect. \odot $= 48^{\circ} 36' 47''$; declin. \odot $= 18^{\circ} 3' 14''$; asc. rect. \odot $47^{\circ} 36' 4''$; declin. \odot $18^{\circ} 19' 46''$; diff. inter asc. rectam \odot & \odot , $1^{\circ} 0' 43''$; diff. inter tempus initii correcti & meridiem, 3 h. $31' 6''$; arcus æquatoris huic tempori competens $= 52' 46' 30'' =$ ang. circ. declinationis per centr. \odot transeuntis cum meridiano loci. Ab hoc subducta differentia inter asc. rect. \odot & \odot remanet pro ang. circ. declinationis per centr. \odot transeuntis cum meridiano $= 51^{\circ} 45' 47''$. Conveniens altit. \odot $= 38' 33''$: angulus circ. decl. cum verticali $= 41' 11''$; parall. alt. $= 47' 50''$, parall. declin. $= 36' 0''$; parall. asc. rect. in circ. parallelo $= 31' 29''$; declin. visa \odot $= 17^{\circ} 43' 46''$; diff. inter declin. visam \odot & declin. \odot $= 19' 28''$; Diff. inter asc. rect. \odot & asc. rect. \odot , reducta ad partes circuli maximi, posita paralleli declinatione $17^{\circ} 44' = 57' 34''$; parallax. asc. rect. $= 31' 29''$: ergo distantia locorum \odot et \odot in hoc circulo parallelo $= 26' 5''$. Si itaque ex $26' 5''$, tanquam basi, et $19' 28''$, tanquam catheto, construitur triangulum rectangulum, hypotenusæ hujus trianguli erit distantia centrorum \odot et \odot ; sed $26' 5'' = 1565''$; cujus quadratum 2449225, et $19' 28'' = 1168''$; cujus quadratum 1364224: summa verò quadratorum $= 3813449$; cujus radix quadrata $= 1953''$ duobus saltem minutis secundis minor summa semidiametrorum apparentium.

Pro Fine Correctio.

20. Hujus momentum supra num. 14. determinatum accidit h. 22, $43' 31''$. Tempus \odot et h. 21, $57' 31''$;

31"; differentia 46'. 0". Ad hanc differentiam motus C in longit. est 28' 25"; incrementum latitud. = 2' 19"; motus \odot in longit. = 1' 51": quomobrem ad h. 22, 43' 31"; longit. C = 51° 35' 13"; latit. C = 38' 36"; longit. \odot = 51° 8' 39"; ascens. rect. \odot = 48° 42' 17"; declin. \odot = 18° 4' 39"; asc. rect. C = 48° 58' 38"; declin. C = 18° 48' 49".

21. Diff. temporis inter finem eclips. et meridiem est h. 1, 16' 29"; quæ, in arcus æquatoris conversa = 19° 7' 15". Diff. inter asc. rect. \odot et C = 16' 21"; asc. rect. C præcedit asc. rect. \odot ; ergo hæc diff. addenda, ut fiat 19° 23' 36", angulus circuli declinat. per centrum C transeuntis cum meridiano. Angulus hic cum latitudine observatorii *Parisiensis* et declin. C profert altitudinem C = 56° 8', et angulum circuli declinationis cum verticali = 23° 4'. Inde consequitur parallaxis altit. = 34' 12"; parall. declinationis 31' 27"; et parall. asc. rect. in circ. parallelo = 13' 24".

22. Reductio ergo et dispositio ascensionum rectarum et declinationum talis erit.

	Asc. rect.				Declin. compet.		
	°	'	"		°	'	"
In δ C	48	30	21	— — —	18	38	59
In δ \odot	48	40	24	— — —	18	4	10
Ad h. 22, 43' 31" \odot	48	42	17	— — —	18	4	39
Ad h. 22, 43' 31" C	48	58	38	— — —	18	48	49
Diff. inter asc. rect. \odot		1	53	Diff. inter decl. \odot			29
In δ C	48	32	14	— — —	18	39	28
Immoti \odot	48	42	14	— — —	18	4	39
Ad h. 22, 42' 31" C	48	58	38	— — —	18	48	39
Diff. <i>a</i>	10	3		Parall. {	32	38	
Diff. <i>b</i>	26	24		declin. {	31	27	
Diff. <i>a</i> reduct.	9	33		Decl. visæ, {	18	6	50 C
Diff. <i>b</i> reduct.	25	5			18	4	39 \odot
					18	17	12 C
	F	2					23.

23. Diff. *a* est distantia ☉ immoti a loco ☾ primo; diff. *b* vero distantia loci ☾ secundi a primo in circulo parallelo, cujus declinatio $18^{\circ} 7'$. Per parallaxes asc. rect. nunc bina ☾ loca mutantur in consequentia, adcoque additis parallaxibus erunt distantia,

$$\odot \text{ immoti} = 9 \quad 33$$

$$\text{☾ in } \delta = 20 \quad 27$$

$$\text{☾ in fin.} = 38 \quad 29$$

Quod si tandem ab his numeris subducatur minor $9' 33''$, relinquitur, pro distantia loci ☾ in δ a sole immoto, $10' 54''$; pro distantia ☾ in fine eclipsis a ☉, $28' 56''$. Differentiæ declinationum visarum, a minima visa sunt, $2' 11''$, et $12' 33''$.

24. Fiat (*Fig. 3.*) *qf* portio circuli paralleli ad declinationem $18^{\circ} 7'$; in eo sit *f* centrum solis immoti; *r* locus lunæ in δ ; *q* locus lunæ in fine eclipsis: quare *rf* = $10' 54''$; *qf* = $28' 56''$. Ad puncta *r* et *q* erigantur perpendiculares *ar* et *qv*; ita ut *ar* sit = $2' 11''$; *qv* = $12' 33''$. Per puncta *v* et *a* ducta recta *mvag* orbitam ☾ visam designabit. Quod si circini apertura sit æqualis summæ semidiametrorum apparentium, hoc casu = $32' 39''$ hæc ex *f* portionem orbitæ *mv*, resecabit, quæ in tempus conversa, et ad momentum finis supra inventi addita, dat finem correctum.

25. Per solos numeros si hoc efficiendum, subducenda primum perpendicularis *ar*, a perpendiculari *vg*, ut habeatur *vz*. Orbita *va* producenda, et ex *f* ducto perpendiculum *fg* demittendum, quibus peractis præcedunt 3 triangula similia; nempe, *azv*, *arn*, et *fng*. Ducto calculo emergit pro *va*, $20' 48''$; pro *an*, $4' 22''$; pro *ng*, $6' 10''$: per consequens,

$vg = 31' 20''$; $gf = 3' 32''$. Cum autem mf sit $= 32' 39''$, erit $mg = 32' 27''$: ergo $mv = mg - vg = 1' 7''$: quæ quantitas in tempus mutata $= 2' 28''$: hoc tempus additum ad tempus finis supra inventum h. 22, 43' 31'', præbet tandem finem eclipsis correctum h. 22, 45' 59''.

Monitum.

Exemplum hoc eam ob causam eligendum duxi, quoniam idem est per quod Dominus *De la Hire* calculi sui præcepta illustravit: operæ igitur pretium erit convenientiam cum præsentī ostendere. Supponitur in calculo *Hireano* momentum conjunctionis secundum tempus verum, h. 21, 57' 15''; quod tamen non satis exactum: nam secundum ipsas tabulas *Ludovicianas* id accidit h. 21, 57' 31''; sicuti nos istud supra statuimus. Levi hoc errore correcto momentum obscurationis maximæ secundum calculum *Hireanum* in ipsis secundis consentit cum nostro, sc. h. 21, 35' 26''; sed initium atque finis necnon quantitas eclipsis exiguo intervallo differunt. Nimirum in isto calculo perpendicularis *LT* (vid. Tab. *Ludovic.* Edit. *Parif.* 1727. p. 48. in *Uſu Tabularum*) prodit ad verum tempus conjunctionis 211; adeoque quantitas eclipsis $= 10$ digit. 49 min. Initium accidit ad h. 20, 27' 29''; finis, h. 22, 43' 23''. Per præceptum *Hireanum* initium istud nulla indiget correctione; quod tamen tunc demum verum est, si error 1 vel $1\frac{1}{2}$ minutorum negligendus censetur. Sin minus, uti res postulat, et probatio correctionis meæ satis ostendit, in *Hireano* calculo correctionis labor quoque suscipiendus. In meo initium prima vice repertum satis exacre quidem con-

consentiret; sed, propter diversas lunæ altitudines in fine et initio, diversos semidiametros apparentes assumpsi, quod Dominus *De la Hire* non fecit; ideoque ut omnia sint paria, semidiameter \odot apprensus, $16' 43''$ in fine et initio constans ponatur; quo casu initium non correctum calculi mei rejicitur ad h. 20, $27' 23''$, finis ad h. 22, $43' 29''$: ergo initium meum antecedit *Hireanum* $6''$; finis vero sequitur eundem eodem intervallo; et quantitas eclipsis, prout eam supra determinavimus, excedit *Hireanam* $7'$.

Cum orbitæ lunæ apparentes, seu potius fictæ in præsentī et *Hireano* calculo non sint revera rectæ, sed curvæ, hac differentia ut in *Hireano* convexitas ejus puncto *L* (vide *alleg.* pag. 48. in Tab. *Ludovic.*) in præsentī vero concavitas puncto *f* (*Fig. 1.*) obijciatur, evidens est perpendicularem *LT*, a cujus longitudine quantitas eclipsis dependet, in *Hireano* calculo esse justo majorem; sicuti in meo eadem perpendicularis, quæ *fg* (*Fig. 1.*) indicatur, justo minor existit: propterea si summa præcisio adhibenda foret, vera eclipsis quantitas inter utrasque intermedia statuenda.

IV. *A Letter from Mr. Chr. Warren, Surgeon at Truro in Cornwall, to John Machin, Esq; Secr. R. S. Prof. Astr. Gresham. containing further Accounts of the Success of injecting medicated Liquors into the Abdomen, in the Case of an Ascites.*

S I R,

Read May 24. 1744. I BEG Leave to return my best Thanks to the SOCIETY in general, and to you, Sir, in particular, for their unanimous Thanks to me, and their Approbation of what I communicated to them in relation to *Jane Roman**, whom I tapp'd and injected for a Dropsy, and yet remains in *statu quo*; tho' she inform'd me Yesterday, that she had labour'd under a Tertian Ague ever since last *January*: Indeed I should not wonder, if, after Nineteen Months Space, she should want the same Operation again, in a Country where Poison [a Dram] is habitually become the common Nutriment, Physician, and Counsellor on all Occasions.

Your kind Communication of Dr. *Hales's* judicious Remarks and Improvement on my Discovery †, does me great Honour and Pleasure; and the more so, as I was so happy as to have discover'd the Use and Efficacy of Injections by means of one Puncture only, on a poor Woman, about Ten Days before I received yours; from whom I drew near fifty Pints of drop-sical Lymph, by an easy Transmutation thereof into

* See *Philos. Transf.* Num. 472, p. 12.

† See *ibid.* p. 20.

an appropriated medicinal Fluid; which was, without any Difficulty, retain'd within the Cavity near two Hours, and, at the Close of the Operation, drawn all off at once, without the least Symptom of a *Syncope* from Inanition; of which I shall beg Leave to acquaint you further, when I see how she holds it, as well as of what else remarkable may occur in the Course of my Practice. I am, with the greatest Regard to the Learned, Worthy Gentlemen of the *Royal Society*,

S I R,

Your most obliged,

Truro, May 12.

1744.

and obedient humble Servant,

Chr. Warwick:

V. *A Letter from John Bevis, M. D. to John Machin, Esq; Secret. R. S. &c. containing some Observations concerning Mercury.*

S I R,

Presented May 4.
1744.

THE Observations to which the inclosed Computations are made by Mr. *Morris* from somewhat more correct Elements than those in Dr. *Halley's* Tables, were carefully taken by myself, with an excellent astronomical Sector of five Feet *Radius*. You will perceive how far I am limited, by my Friend's Request; so must intreat you, if you think it worth while, to inform the

the *Royal Society*, that *Mercury's* Motion has not been at all disturb'd, [by the late *Comet*] to do it in what manner you shall think best. I am,

S I R,

Your obedient,

Wednesday, May 23.

1744.

humble Servant,

J. Bevis.

1744, May 17. The apparent Right Ascension } ° ' "
 of ϵ Geminorum, according to Dr. Bradley's } 97 2 10
 Observations, - - - - - }
 Declination, - - - 25 21 30

May.	Appt. time.	Long. ϕ .	Latitude.	Comp. Rt. Asc.
	h m s	° ' "	° ' "	
15	8 31 15	1128 56 51	1 57 58 N.	88 50 7
17	8 26 6	11 1 16	1 44 45	91 7 41
18	9 4 40	1 59 30	1 36 40	92 11 53
19	8 41 00	2 51 38	1 28 7	93 9 12

May.	Comp. Declin.	Obsd. Rt. Asc.	Obsd. Declin.	Error Comput.	
	° ' "	° ' "	° ' "	Rt. Asc.	Declin.
15	25 26 12	88 49 20	25 26 20	+ 47	- 8
17	25 13 00	91 7 4	25 12 56	+ 37	+ 4
18	25 4 15	92 11 10	25 4 27	+ 43	- 12
19	24 54 43	93 8 20	24 54 56	+ 52	- 13

VI. *A Rupture of the Navel, communicated to the ROYAL SOCIETY by H. W. Taube, (Dove) Surgeon of the Pearl Man of War.*

Read May 24.
1744.

ANN *Stubbensfull* had a very hard Labour 17 Years before her Death, and a little Rupture appeared in her Navel, and in the next Labour it increased; which she endeavoured to cure by a Bandage, but in vain; so it continued to increase more and more.

The first time I was called to her, was upon account of a Wound at *Lett. d.* where it looked as if it would mortify; which I cured, but left a Place open as big as Half a Crown, from which a great Quantity of Water would sometimes run out; but getting Cold, it stopped; and the whole *Saccus* was very much inflamed (This Wound was formerly made by uneasy Trusses). I called on the late Mr. *Amiand*; who told me, he had seen in a Work-house a Rupture of the same Kind, but not so big.

She had once an *Obstructio alvi* for fifteen Days; and nothing would do, till I order'd her a Glyster of Tobacco boil'd with Urine. The late Dr. *James Douglass* saw her; and he and I agreed to open her after her Death; to which she consented. At last, she died maniacal. Dr. *Douglass* and I would have open'd her; but the Obstinacy of her Children was so great, that they would not agree to it till the fourth Day after her Death, which happened in *July*, when the Putrefaction was so great, that Dr. *Douglass*, nor I, nor any body else, could remain by her; and so were obliged, to our great Sorrows, to desist.

References

References to the Figures, see TAB. II. Fig. 4 and 5.

- a.* The Circumference nearest to the Belly 36 Inches and an half.
- b.* The thickest Part, 38 Inches.
- c.* From *a* to *c*, the Length, 34 Inches.
- d.* The Wound.
- e.* Several great Protuberances.

VII. *A Letter from Mr. Wm. Watson, F.R.S. to the ROYAL SOCIETY; containing further Remarks concerning Mushrooms: Occasioned by the Reverend Mr. Pickering's F. R. S. Paper in the preceding Transact. p. 96. with Observations upon the poisonous Faculty of some Sorts of Fungi.*

Gentlemen,

Read May 31. 1744. I HOPE I shall have the Reverend Mr. *Pickering's* Excuse, if I lay before you a few further Observations upon his Papers concerning *Mushrooms*.

With regard to the Seeds of *Mushrooms*, although they were never shewn to the ROYAL SOCIETY before, the Fact was known to many Members thereof: for the industrious *Micheli* did not only raise Mushrooms from their Seeds, but has, in his Tables, shewn the daily Progress from their first Point of Vegetation, even to their perfect State.

The *Fungus porosus crassus magnus* is not the Mushroom usually raised in *England* for the Table,

as this Gentleman did imagine ; that Name being given by *John Bauhin*, in the third Volume of his History (p. 833.), to a Species which is to be distinguished from all other *Fungus's*, by the inferior Substance not being divided into *Lamellæ*, or (what we call in *England*) Gills ; but has, in lieu thereof, a great many *Papillæ* ; and being of a greenish-yellow Colour. But what is raised in *England* (of which this learned Gentleman brought several Samples to the SOCIETY) is the *Fungus campestris albus supernè, infernè rubens*, of *John Bauhin*, which differs *toto cælo* from the former, and which Dr. *Dillenius* enumerates among the Species of *Boletus* ; whereas the latter is a Species of *Amanita*.

I must beg Leave to differ from this Gentleman likewise, in regard to the Use of the Ring, which surrounds the Stalk of this *Mushroom*. He imagines it placed there, by the wise Author of Nature, to break the Fall of the Seeds when ripe ; whereby those light Bodies may be preserved from the Fury of the Winds, in order to the abundant Propagation of their Species. I have Reason to believe, that those Seeds, which fall upon this Ring, fall there by Accident ; and adhere there only from the Viscosity, whereby they are intangled. But, before I examine this Matter, give me Leave to make a few Observations upon the Oeconomy of this Plant. The *Fungi*, then, are of that Class of Vegetables, which are ranged, by that most skilful Botanist *Linnaeus*, under the Appellation of *Cryptogamia*, or those which perform their Frustrification in secret. Under this Head we find the Fig-tree, all the Species of Fern, Mosses, Mushrooms, and a few others, whose Flowering and Seeding are observed.

observed with more Difficulty, than in those we usually call the more perfect Plants. In some of this Class, the Fructification, notwithstanding the great Assistance furnish'd to the modern Botanists by Microscopes, which the ancient were wholly destitute of, remains yet undiscover'd. This Plant then being of this Class, almost all those whose Stems are thick and fleshy, as well as their Umbels, have a Ring upon their Stem; from which, when the Plant is young, and until it arrives at a flowering State, there arises a Membrane, which connects the Rim of the Umbel to the Stem, and preserves the under Part of the Plant in this State: But, when this is over, the Umbel, which before was almost of an hemispherical Figure, growing larger, and the Membrane not giving way, is loosened from the Rim of the Umbel, and adheres only to the Stem. Soon after this State, the Seeds ripen, and the Umbel, losing its former Figure, commences almost a Plane; and the Plant in this State is sold in our Markets, by the Name of *Flaps*. Now, when the Umbel is of this Figure, the Seeds, being perfectly ripe, must fall naturally upon the whole Space the Umbel covers (which *Micheli* observed, by placing Leaves of Trees under them); and, upon the Ring, as well as any other Part; though I have Reason to believe not more. As for those Species of *Fungi* whose Stems are thin, and whose Umbels are soft, and more ductile, they need not, nor have they, this Ring or Membrane; because, in their tender State, the Rims of their Umbels clap themselves quite close to the Stalk, in the Form of a contracted *Umbrella*; and expand as the others do, when their Seeds.

Seeds are ripe : nevertheless the Species of this Tribe are as numerous as the former.

I now come to consider how far the Poison of *Mushrooms* can possibly proceed from Animalcules : But, first, give me Leave to doubt, whether or no any Person was ever injured from eating the common *Mushroom*, or *Amanita* ; unless such Accident may have proceeded either from eating too many at once, and thereby overloaded the Stomach ; or from some particular Dislike in the Constitution ; as we sometimes see, even with regard to Honey, Cheese, and some of the most innocent Parts of our Diet ; but which, notwithstanding this, are by no means to be rank'd among Poisons. If there were many Instances of their being pernicious, such must frequently occur to the Practitioners in Physic, on account of the vast Quantity annually consumed in *London* ; but I don't remember to have even heard of any such Accident ; but many Instances occur of the noxious Quality of many of the other Species of this Tribe : Nor is it at all wonderful, that the different Subjects of this Class of Vegetables should differ in their Effects more than those of the more perfect Kind. The Roots of Carrot, Parsnep, and many others of the umbelliferous Class, are daily used as Food ; but the Water-Hemlock, and *Lobel's Oenanthe*, though of the same Class, are most certain Poisons.

Here I must observe what Pains have been taken by Naturalists, to distinguish the useful from the pernicious Kinds. Among the *Romans*, the *Boletus* mentioned by *Juvenal*, on account of the Death of the Emperor *Claudius*, is sufficiently described by *Pliny* ; but, among the later Writers, *Carolus Clusius* was

was of the first of those, who, about the Middle of the sixteenth Century, being tired with the Critics and Commentators of the Time he lived in, presumed to believe, that the Whole of Knowledge was not confin'd to the Writings of the *Greek, Roman,* and *Arabian* Physicians; because, from the Revival of Letters in the Western World to his Time, nothing was regarded, as of any Importance, but what was dignified with the Authority of Antiquity: And hence it came to pass, that when the Clouds of Ignorance began to disperse, the *Epocha* of Commentators took Place; but many of the Descriptions of the Plants of *Theophrastus, Dioscorides,* and *Pliny,* were so very deficient, that little Light could be acquired therefrom; especially from this last Author, who is to be consider'd as the only *Roman* Naturalist that we have handed down to us; and it is no Wonder, if, among the vast Variety of Subjects that this most admirable Historian treats of, he is, in many Instances, rather to be consider'd as an Enumerator, than as a Describer: I shall only mention the imperfect Sketches he has left us of *Silva, Geum, Molon,* among the many others.

There arose, I say, such Heats and Disputations among the Critics upon those Authors, very often about Trifles, that they rather increased than diminished the Ignorance of those Times. This excellent *Clusius*, finding that a thorough Knowledge of Nature was necessary, not only to understand rightly the Ancients, but to lay the Foundation of future Knowledge, was desirous to join careful Observations of his own to those which were to be acquired from Books. How much he travell'd, and
what

what Progress he made in this Undertaking, his many valuable Works are the best Testimony. Among them, his History of *Fungus's* bears not the *least* Character; he therein enumerates a great Variety, not only of the esculent, but noxious Kinds; but, as the different Appellations of every Species was not, at that time, much consider'd, he gives no other *Synonyms* to either Class, than that of, *viz. Esculentorum primum Genus, Noxiorum decimum Genus*, and such-like. But this Want of specific Names has been sufficiently supplied by *John* and *Caspar Baubin*, *Ray*, *Morison*, *Tournefort*, *Vaillant*; but, above all, by *Dillenius*, in his *Catalogus Giffensis*, and by *Micheli*, in his *Nova Plantarum Genera*. In most of these Authors we find Instances of mischievous Effects from the pernicious Kinds; which Property some of them have equal to *Opium*, *Aconite*, or *Henbane*; but how far this Property proceeds from *Animalcules*, the following Instance will sufficiently demonstrate. We have a Sort growing in *England*, called, by *Caspar Baubin*, *Fungus albus acris*; which *Monsieur Tournefort* has rightly observed stimulates the Tongue, and is almost as sharp as tho' it were steep'd in Spirit of Nitre; and, being rubbed upon Paper dyed blue with Turnsole, turns it as red as any violent acid Spirit will. This caustic Quality remains even after the *Fungus* is dry. We need make no further Inquiry for the Cause of the Poison in this Plant; the above-mentioned is a sufficient Criterion. *John Baubin* likewise tells you, that after having handled this *Fungus*, he rubbed his Eyes by Accident, and brought on a violent Irritation upon his Eye-lids. *Caspar Baubin* mentions a Sort which
kills

kills the very Flies. *Micheli* describes a Species, which, upon eating them, almost kill'd the Painter he usually employed, and an old Woman, the Painter's Mother. This Man, being sent by the Author to delineate some of these *Fungus's*, and being taken with their Appearance, order'd some of them to be fried, and he and his Mother eat thereof; but were, in about Two Hours, seized with violent Pains in their Bowels, from which they were with great Difficulty relieved. I might produce many other Instances of this Sort; but the above, I believe, are sufficient.

I presume I have set these Matters in a clear Light; but, fearing I have already taken up too much of your Time, I conclude myself,

Gentlemen,

Your most devoted,

London, May 12.

1744.

humble Servant,

W. Watfon.

VIII. *Two Letters from Mr. Thomas Preston to Mr. Joseph Ames, F. R. S. concerning the Island of Zetland.*

Dear Sir,

Zetland, Jan. 31. 1744³/₄.

*Read May 31.
1744.*

THIS Island has very rarely any Correspondence with the rest of the World for the Six Winter Months; I should say, for

H

Six

Six of the Winter Months; for the Year may be said to contain ten Months of Winter, and two of cold raw Weather. I thought it very absurd, to hear the Inhabitants complain of Heat, at the same Instant that I complain'd of Cold, and wish'd for a great Coat. They are so accusom'd to stormy bad Weather, that they will venture to Sea in small Boats, when you would not venture to cross the *Thames*.---- I shall give you a short Description of this Island.

It is the Northermost belonging to *Scotland*, situated between the Latitudes of 60 and 61 Degrees: its Length is, *N.* and *S.* 60 Miles; its Breadth 30; and so divided into Head-lands and smaller Islands, Creeks, Bays, Inlets, and Coves, &c. that you cannot place a Compass on any the most inland Parts of its Chart that shall be Two Miles from the Sea; which makes it extreme difficult to make a good Draught of the Island; of which there is no Chart extant worth naming.

During my long Stay, I have devoted some of my (otherwise) idle Time to the making of Observations, and surveying the Place; by which, and suitable Informations, &c. gain'd from the Inhabitants, I have attempted a Sea Chart, which, I flatter myself, may be of Use to our Navigators; who are all Strangers to the many safe and good Harbours in this Island; and several of them capable of receiving many large Ships.

The Land is wild, barren, and mountainous, nor is there so much as a Tree or Bush to be seen. The Shores are difficult, and, in many Places, inaccessible, rude, steep, and iron-like; the Sight of which strikes the Mind with Dread and Horror; and such monstrous

stuous Precipices, and hideous Rocks, as bring all *Broddingnag* before your Thoughts.

I doubt not but you are now listening out for some Account of Curiosities of Art and Nature; which most here are Strangers to; and yet this Island is not quite barren. In Winter the Sun sets soon after it rises; and in Summer it rises again soon after its Setting; so that the Nights at that Season are near as light as the Day; as, on the contrary, the Day in *December* is near as dark as the Night. About the Solstice, we see, almost every Night, the *Aurora Borealis*, as I think you call it; but we Seamen, the Northern Lights; which spreads a broad glaring Appearance over the whole Northern Hemisphere, and looks somewhat terrifying to them that are not used to it.

I shall only just mention, that a Comet has appear'd to us for some time from the West, large and plain to the naked Eye.

S I R,

Leith, May 12. 1744.

IN my last from *Zetland*, I gave you a short Account of that Country. I shall now give you some more Particulars, as they occur to my Memory.

The Island is called by the *Dutch*, *Hitland*; by Us, commonly, *Shetland*; but the proper Name of it is *Zetland*; wherein there are thirty Parish-Churches, and about eighty Gentlemens Houses, besides the Towns of *Lerwick* and *Scalloway*: It was first inhabited by the *Pights* or *Picts*, who were driven out by the *Danes*. *Christian*, King of *Den-*

mark and *Norway*, whose Daughter *Margaret* was given in Marriage to King *James* the Third of *Scotland*, in the Year 1468, agreed, That the Islands of *Orkney* and *Zetland* should remain in the Possession of the said King *James*, until he had paid to him 50,000 *Rhenish* Florins for his Daughter's Dower; and *Christian* afterwards, upon the Birth of a young Prince his Grandson, call'd *James*, renounced his Title to the said Island, in Favour of King *James*; which has ever since belonged to *Scotland*.

The Longitude of *Zetland* differs but little from the Eastern Part of *Scotland*; and its Latitude is from 60 to 61 Degrees North: The longest Day is near twenty Hours. I have read a very small Print in my Chamber at Midnight with the Windows shut. The Air is temperate, considering the Country lies so far North; and agrees tolerably well with those that can endure Cold and a thick Fog. I must own, I have not found the Winters so cold as in some other Parts of *Great Britain*; nor are the Summers near so warm; for which, indeed, the Length of the Days makes some amends. I have heard, that some Fishermen have affirmed, that at Sea they have seen the Sun's Body at Midnight: But that is impossible; since the Sun in the shortest Day in *December* is four Hours above the Horison; and it must certainly be depressed as long under it in *June*. The Winters are more subject to Rain than Snow; nor do the Frost and Snow continue so long on the Ground, as upon the main Land of *Britain*; yet I saw it wholly cover'd with Snow the 20th of *May* the last Year. The Winds, during the long Winter season, continue to blow most boisterously generally between the South and the

the West ; which occasions many Shipwrecks. There have been three or four on the Island in the Time I was there.

The Land is mountainous and moorish, abounding with Moss and Heather ; under which they dig Peat (or Turf) for Firing ; under that is hard Rock.

Their Horses are very little, but strong, and well mettled, which they call *Shelties*. Their Oxen, Swine, and Sheep, of which last they have Plenty, and their Cattle of all Kinds, are small. The Price of a Horse is a Guinea ; an Ox somewhat less ; a Sheep half a Crown ; a Calf Six-pence. Rabbits they have in some Parts of the Country. Frogs and Toads there are none ; nor, perhaps, any poisonous Animals. There are many Otters, which they call *Tikes* ; and Seals, which they call *Selkies* : Sometimes there are many young Whales, which they call *Pellacks*, or Spout-Whales : They run into the Creeks, and so intangle themselves among the Rocks, that they are cast on the Shore, or easily taken.

There are Plenty of Sea-weeds, called *Tangle*, growing on the Rocks, of which might be made *Kelp* for the making of Soap. There is Plenty of Shell-fish of most Kinds : Their Oysters are the best I ever eat. In the Sea they catch Cod, Ling, Haddock, Whittings, Scate, Turbut, Herrings, Cole-fish, Flukes, Trouts, &c.

There are many Sorts of Wild-fowl ; namely, the *Dunter* Goose, *Clark* Goose, *Solan* Goose, Swans, Ducks, Teal, Whaps, Toists, Lyres, Kittiwaks, Maws, Plovers, Scarfs, &c. there is likewise the *Ember-Goose*, which is said to hatch her Egg under her Wing : This is certain, that none ever saw her on the Land, or out of the Water ;
and

and that they have a Cavity or hollow Place under one of their Wings only, capable of containing a large Egg.

There are very large Eagles, which they call *Earns*, which prey upon the young Lambs, &c. There is a Law in Force, That whoever kills one of these Eagles, is to have a Hen out of every House in the Parish wherein it is killed (tho' it is never now demanded).

I don't doubt but that there are Mines of Silver, Tin, and Lead, in the Country ; for several Sorts of Minerals are to be found : And I have been credibly informed, that Pieces of pure massy Silver, of considerable Bigness, have been turned up by the Plough : But such Treasures are neglected, or not improved, through the Poverty or Carelessness of the Proprietors. In many Places, Marl, Quarries of excellent Freestone, Lime-stone, and Slate, are found, and some Veins of Marble.

Sometimes there are cast up by the Sea, Timber, Pieces of Wreck, Hogsheds of Wine, Brandy, &c. which are frequently grown over with the Shells of a Fish called *Cleck Geese*, which I take to be the *Conchæ anatiferae* ; and sometimes *Sperma Ceti*, Ambergrise, Water-Sponges, and Camshells (*Os Sepiæ*) are found on the Shore.

There is no Forest or Wood, nor so much as a Tree, or hardly a Bush of any Sort in the whole Country, except in some Gentlemens Gardens ; and those never dare to peep over the Garden-Wall, for the Blast of the North Wind.

The Country is so divided by the Sea, that it cannot be expected there should be in it any Rivers ; yet there are many small Brooks, and little Runs of
Water

Water call'd *Bourns*, and *Loches* or Lakes; many of which afford Trouts; and I have eaten excellent Cod-fish, that have been taken in one of these *Loches* or Lakes of perfect fresh Water, which has been very near the Sea.

The People are generally civil, sagacious, of a ready Wit, and of a quick Apprehension, piously inclin'd, much given to Hospitality, civil and liberal in their Entertainments, and exceeding kind to Strangers; which I may say indeed from Experience; for I never met with more Civility in any Part of the World.

They are generally of a dark Complexion, personable, and comely enough. The Women are lovely; and the Gentry of them go well-drest, are genteel in Carriage, well-behav'd in Company, and smart and pleasant in Conversation, even to a Miracle, considering they live in such a remote Island, which has so little Correspondence with the rest of the World: They delight more in the Conversation of Men, than in the common Tittle-tattle of their own Sex; they are Strangers to Plays, Opera's, Masquerades, Balls, Assemblies, Set Visiting-Days, extravagant Dress, Gallantry, &c. and are free from those fashionable Vices which so much disgrace their Betters: In a Word, they are modest Virgins, and virtuous Wives: Adultery is not so much as known among them: With the common Sort Fornication sometimes happens; but their Constancy is such, that they are sure to marry each other after; nevertheless, if a Child happens to come in less than nine Months after the Marriage, they are both obliged, by the Law of *Scotland*, to

do Penance in the *Kirk*. This heinous Sin the pious Priest calls *Antenuptial Fornication*.

The Country is most commodious for Navigation; which makes me wonder it has been so long neglected, and that we have not even so much as a Map of it. There are more than twenty safe Harbours, of easy Access, capable of receiving large Ships; the most remarkable of which are *Lerwick* or *Bressay* Sound; *Dura Voc* and *Balta* Sound on the East Side, and *Scalloway*, *Olisvoe*, and *Valley* Sound on the West Side of the Country. The Coast is all high and bold, and may be seen many Leagues from Sea. There are no Sands round the whole Island, and but few sunken Rocks, and those very near the Shore; except one dangerous Shoal on the West Side, called *Have de Grind*, and some Rocks on the N. W. above Water; both which, as well as the whole Island, I have exactly survey'd; and I think I may say, without Vanity, that I can produce a very good Map of *Zetland*, which, I believe, may be of good Service to Navigation, especially this War-Time. --- I fear I have now tried your Patience with my long incoherent Epistle: I shall therefore conclude with my kind Love to all Friends; who am,

Dear Sir,

Your most affectionate,

Humble Servant,

Tho. Preston.

N. B. The Author has lately published a very accurate Map of these Islands.

IX. *Occultatio Jovis à Luna, obs. Londini.*

Read June 7.

1744.

Notante Horologio.

d h l //

1744. June 6. 11 13 40 Immerfio centri *Jovialis*, fat certe.
 35 14 α *Serpentaria* culminat.
 43 15 Emerfio centri, raptim inter nubes.
 Tubo 12 Ped.

J. Bevis.

X. *A letter from Mr. Abraham De Moivre, F. R. S. to William Jones, Esquire, F. R. S. concerning the eafiest method for calculating the value of annuities upon lives, from tables of observations.*

S I R,

Presented June 7.

1744.

YOU may remember, that some time after the printing of the second edition of my book of annuities on lives, you told me, that it seem'd strange to you, that, considering I had demonstrated the chief propositions in the book, I had neglected to demonstrate the theorem, which is found in page 86. line 12. which, you said, of all the rest, appear'd to you the most curious; I answer'd, that, as the demonstration depended upon a principle which was not commonly known

known, I was afraid that the publishing of it would have swell'd the book too much; for this reason especially, that many corollaries were annexed to it. However, I promised to send it you in a short time; but desired you to let me know, whether you thought it deserv'd a place in the Philosophical Transactions. I now discharge my promise; and expect, with impatience, the favour of your opinion. I am, with a very particular regard,

S I R,

Your most humble,

and most obedient Servant,

A. De Moivre.

A short method of calculating the value of annuities on lives, from tables of observations.

ALTHO' it has been an established custom, in the payment of annuities on lives, that the last rent is lost to the heirs of the late possessor of an annuity, if the person happens to die before the expiration of the term agreed on for payment, whether yearly, half-yearly, or quarterly: nevertheless, in this treatise I have suppos'd, that such a part of the rent should be paid to the heirs of the late possessor, as may be exactly proportion'd to the time elaps'd between that of the last payment, and the
very

very moment of the life's expiring; and this by a proper, accurate, and geometrical calculation.

I have been induced to take this method, for the following reasons; first, by this supposition, the value of lives would receive but an inconsiderable increase; secondly, by this means, the several intervals of life, which, in the tables of observations, are found to have uniform decrements, may be the better connected together. It is with this view that I have framed the two following problems, with their solutions.

PROBLEM I.

To find the value of an annuity, so circumstanced, that it shall be on a life of a given age; and that, upon the failing of that life, such a part of the rent shall be paid to the heirs of the late possessor of an annuity, as may be exactly proportioned to the time intercepted between that of the last payment, and the very moment of the life's failing.

SOLUTION.

LET n represent the complement of life, that is, the interval of time between the given age, and the extremity of old-age, suppos'd at 86.

r the amount of 1*l.* for one year.

α the logarithm of r .

P the present value of an annuity of 1*l.* for the given time.

Q the value of the life sought.

$$\text{Then } \frac{1}{r-1} - \frac{P}{\alpha r} = Q.$$

DEMONSTRATION.

For, let z represent any indeterminate portion of n . Now the probability of the life's attaining the end of the interval z , and then failing, is to be expressed by $\frac{z}{n}$, (as shewn in page 77, edit. 1. and in page 115, edit. 2. of my book of annuities upon lives) upon the supposition of a perpetual and uniform decrement of life.

But it is well known, that if an annuity certain, of 1/. be paid during the time z , its present value will be $P = \frac{1-r^z}{r-1}$ or $\frac{1}{r-1} - \frac{1}{r-1 \times r^z}$.

And, by the laws of the doctrine of chances, the expectation of such a life, upon the precise interval z , will be expressed by $\frac{z}{n \times r - 1} - \frac{z}{n r^2 \times r - 1}$; which may be taken for the ordinate of a curve, whose area is as the value of the life required.

In order to find the area of this curve, let $p = n \times r - 1$; and then the ordinate will become $\frac{z}{p} - \frac{z}{p r^2}$, a much more commodious expression.

Now it is plain, that the fluent of the first part is $\frac{z}{p}$: but as the fluent of the second part is not so readily discover'd, it will not be improper, in this place, to shew by what artifice I found it; for I do not know, whether the same method has been made use of by others: all that I can say, is, that I never
had

had occasion for it, but in the particular circumstance of this problem.

Let, therefore, $r^z = x$; hence $z \log. r = \log. x$; therefore $z \log. r = (\text{fluxion of the } \log. x =) \frac{\dot{x}}{x}$, or $az = \frac{\dot{x}}{x}$; consequently $z = \frac{\dot{x}}{ax}$, and $\frac{z}{r^z} = \frac{\dot{x}}{axx}$; but the fluent of $\frac{\dot{x}}{axx}$ is $(-\frac{1}{ax} =) -\frac{1}{ar^z}$; and therefore the fluent of $-\frac{z}{pr^z}$ will be $+\frac{1}{p^2 r^z}$.

The sum of the two fluents will be $\frac{z}{p} + \frac{1}{p^2 r^z}$; but, when $z = 0$, the whole fluent should be $= 0$; let therefore the whole fluent be $\frac{z}{p} + \frac{1}{p^2 r^z} + q = 0$.

Now, when $z = 0$, then $\frac{z}{p} = 0$, and $\frac{1}{ap^2 r^z}$ becomes $\frac{1}{ap}$ (for $r^z = 1$), consequently $\frac{1}{ap} + q = 0$; and $q = -\frac{1}{ap}$: therefore the area of a curve, whose ordinate is $\frac{z}{p} - \frac{z}{pr^z}$ will be $(\frac{z}{p} - \frac{1}{ap} + \frac{1}{ap^2 r^z} =) \frac{z}{p} - \frac{1}{r^z - 1} \times \frac{1}{ap}$

But $P = \frac{1}{r-1} - \frac{1}{r-1 \times r^z}$; therefore $1 - \frac{1}{r^z} = \frac{r-1}{r-1} \times P$, and the expression for the area becomes $\frac{z}{p} - \frac{P}{az}$: And putting n instead of z , that area, or the value of the life, will be expressed by $\frac{1}{r-1} - \frac{P}{an}$.

Q. E. D.

Those

Those who are well versed in the nature of logarithms, I mean those that can deduce them from the doctrine of fluxions and infinite series, will easily apprehend, that the quantity here called α , is that which some call the hyperbolic logarithm; others, the natural logarithm: it is what Mr. *Cotes* calls, the logarithm whose modulus is 1: lastly, it is by some called *Neper's* logarithm. And, to save the reader some trouble in the practice of this last theorem, the most necessary natural logarithms, to be made use of in the present disquisition about lives, are the following:

If $r = 1.04$, then will $\alpha = 0.0392207$.

$r = 1.05$, - - - $\alpha = 0.0487901$.

$r = 1.06$, - - - $\alpha = 0.0582589$.

It is to be observed, that the theorem here found, makes the values of lives a little bigger, than what the theorem found in the first problem of my book of annuities on lives, does; for, in the present case, there is one payment more to be made, than in the other; however, the difference is very inconsiderable.

But, altho' it be indifferent which of them is used, on the supposition of an equal decrement of life to the extremity of old-age; yet, if it ever happens, that we should have tables of observations, concerning the mortality of mankind, intirely to be depended upon, then it would be convenient to divide the whole interval of life into such smaller intervals, as, during which, the decrements of life have been observed to be uniform, notwithstanding the decrements in some of those intervals should be quicker, or slower, than others; for then the theorem here
I
found

found would be preferable to the other; as will be shewn hereafter.

That there are such intervals, Dr. *Halley's* tables of observations sufficiently shew; for instance; out of 302 persons of 54 years of age, there remain, after 16 years (that is, of the age of 70) but 142; the decrements from year to year having been constantly 10; and the same thing happens in other intervals; and it is to be presumed, that the like would happen in any other good tables of observations.

But, in order to shew, in some measure, the use of the preceding theorem, it is necessary to add another problem; which, tho' its solution is to be met with in the first edition of my book of annuities on lives, yet it is convenient to have it inserted here, on account of the connexion that the application of the preceding problem has with it.

In the mean time, it will be proper to know, *What part of the yearly rent should be paid to the heirs of the late possessor of an annuity, as may be exactly proportioned to the time elapsed between that of the last payment, and the very moment of the life's expiring.* To determine this, put A for the yearly rent; $\frac{1}{m}$ for the part of the year intercepted between the time of the last payment, and the instant of the life's failing; r the amount of 1% at the year's end: then will $\frac{1}{r^m - 1} A$, be the sum to be paid.

PROBLEM II.

To find the value of an annuity for a limited interval of life, during which the decrements of life may be considered as equal.

SOLUTION.

LET a and b represent the number of people living in the beginning and end of the given interval of years.

s represent that interval.

P the value of an annuity certain for that interval.

Q the value of an annuity for a life supposed to be necessarily extinct in the time s ; or (which is the same thing) the value of an annuity for a life, of which the complement is s .

Then $Q + \frac{b}{a} \times \overline{P-Q}$ will express the value required.

DEMONSTRATION.

For, let the whole interval between a and b be fill'd up with arithmetical mean proportionals; therefore the number of people living in the beginning and end of each year of the given interval s will be represented by the following series; viz.

$$a. \frac{sa-a+b}{s} . \frac{sa-2a+2b}{s} . \frac{sa-3a+3b}{s} . \frac{sa-4a+4b}{s} . \&c. \text{ to } b.$$

Consequently, the probabilities of the life's continuing during 1, 2, 3, 4, 5, &c. years will be expressed by the series,

$$\frac{sa-a+b}{sa} . \frac{sa-2a+2b}{sa} . \frac{sa-3a+3b}{sa} . \frac{sa-4a+4b}{sa} . \&c. \text{ to } \frac{b}{a}.$$

Wherefore,

Wherefore, the value of an annuity of 1 *l.* granted for the time *s*, will be expressed by the series

$$\frac{sa-a+b}{sar} + \frac{sa-2a+2b}{sar^2} + \frac{sa-3a+3b}{sar^3} + \frac{sa-4a+4b}{sar^4},$$

ℳc. to $+\frac{b}{ar^s}$; this series is dividible into two other series's, *viz.*

$$1st. \frac{s-1}{r} + \frac{s-2}{r^2} + \frac{s-3}{r^3} + \frac{s-4}{r^4}, \text{ } \mathcal{C}c. \text{ to } + \frac{s}{sr^s}$$

$$2d. \frac{b}{a} \times \frac{1}{sr} + \frac{2}{sr^2} + \frac{3}{sr^3} + \frac{4}{sr^4}, \text{ } \mathcal{C}c. \text{ to } \frac{s}{sr^s}.$$

Now, since the first of these series's begins with a term whose numerator is $s-1$, and the subsequent numerators each decrease by unity; it follows, that the last term will be $=0$; and, consequently, that series expresses the value of a life necessarily to be extinct in the time *s*. The sum of this series may be esteem'd as a given quantity; and is what I have expressed by the symbol \mathcal{Q} in problem 1.

The second series is the difference between the two following series's,

$$\frac{b}{a} \times \frac{1}{r} + \frac{1}{r^2} + \frac{1}{r^3} + \frac{1}{r^4} + \mathcal{C}c. \text{ to } r^s$$

$$\frac{b}{a} \times \frac{s-1}{sr} + \frac{s-2}{sr^2} + \frac{s-3}{sr^3} + \frac{s-4}{sr^4} \mathcal{C}c. \text{ to } + \frac{s-s}{sr^s}.$$

Where, neglecting the common multiplier $\frac{b}{a}$, the first series is the value of an annuity certain to continue *s* years; which every mathematician knows how to calculate, or is had from tables already compos'd for that purpose: this value is what I have call'd \mathcal{P} ; and the second series is \mathcal{Q} .

K

Therefore

Therefore $Q + \frac{b}{a} \times \overline{P-Q}$ will be the value of an annuity on a life for the limited time. *Q. E. D.*

It is obvious, that the series denoted by Q , must of necessity have one term less than is the number of equal intervals contain'd in s ; and therefore, if the whole extent of life, beginning from an age given, be divided into several intervals, each having its own particular uniform decrements, there will be, in each of these intervals, the defect of one payment; which to remedy, the series Q must be calculated by problem 1.

EXAMPLE.

To find the value of an annuity for an age of 54, to continue 16 years, and no longer.

IT is found, in Dr. *Halley's* tables of observations, that a is 302, and b 172: now $n=s=16$; and, by the tables of the values of annuities certain, $P=10.8377$; also (by problem 1.) $Q=(\frac{1}{r-1}-\frac{P}{an})=6.1168$. Hence it follows (by this problem), that the value of an annuity for an age of 54, to continue during the limited time of 16 years, supposing interest at 5 per cent. per annum, will be worth $(Q + \frac{b}{a} \times \overline{P-Q})$ 8.3365 years purchase.

From Dr. *Halley's* tables of observations, we find, that from the age of 49 to 54 inclusive, the number of persons, existing at those several ages, are, 357, 346, 335, 324, 313, 302, which comprehends a space of five years; and, following the precepts before laid down, we shall find, that an annuity

annuity for a life of 49, to continue for the limited time of 5 years, interest being at 5 *per cent. per annum*, is worth 4.0374 years purchase.

And, in the same manner, we shall find, that the value of an annuity on life, for the limited time comprehended between the ages of 42 and 49, is worth 5.3492 years purchase.

Now, if it were required to determine the value of an annuity on life, to continue from the age of 42 to 70, we must proceed thus :

It has been proved, that an annuity on life, reaching from the age of 54 to 70, is worth 8.3365 years purchase ; but this value, being estimated from the age of 49, ought to be diminished on two accounts : First, because of the probability of the life's reaching from 49 to 54, which probability is to be deduced from the table of observations, and is proportional to the number of people living at the end and beginning of that interval, which, in this case, will be found 302 and 357 : The second diminution proceeds from a discount that ought to be made, because the annuity, which reaches from 54 to 70, is estimated 5 years sooner, *viz.* from the age of 49, and therefore that diminution ought to be expressed by $\frac{1}{7}$; so that the total diminution of the annuity of 16 years will be expressed by the fraction $\frac{302}{357}$, which will reduce it from 8.3365 years purchase to 5.5259 ; this being added to the value of the annuity to continue from 49 to 54, *viz.* 4.0374, will give 9.5633, the value of an annuity to continue from the age of 49 to 70. For the same reason, the value 9.5633, estimated

from the age of 42, ought to be reduced, both upon account of the probability of living from 42 to 49, and of the discount of money for 7 years, at *5 per cent. per annum*, amounting together to 3.8554, which will bring it down to 5.7079; to this adding the value of an annuity on a life to continue from the age of 42 to 49, found before to be 5.3492, the sum will be 11.0571 years purchase, the value of an annuity to continue from the age of 42 to 70.

In the same manner, for the last 16 years of life, reaching from 70 to 86, when properly discounted, and also diminished upon the account of the probability of living from 42 to 70, the value of those last 16 years will be reduced to 0.8; this being added to 11.0571 (the value of an annuity to continue from the age of 42 to 70, found before), the sum will be 11.8571 years purchase; the value of an annuity to continue from the age of 42 to 86; that is, the value of an annuity on a life of 42; which, in my tables, is but 11.57, upon the supposition of an uniform decrement of life, from an age given to the extremity of old-age, supposed at 86.

It is to be observed, that the two diminutions, above-mention'd, are conformable to what I have said in the corollary to the second problem of the first edition, printed in the year 1724.

Those who have sufficient leisure and skill to calculate the value of joint lives, whether taken two and two, or three and three, in the same manner as I have done the first problem of this tract, will be greatly assisted by means of the two following theorems:

If

If the ordinate of a curve be $\frac{x}{r^2}$; its area will be $\frac{x}{a^2} - \frac{x}{a^2 r^2} - \frac{x}{a r^2}$.

If the ordinate of a curve be $\frac{x^2}{r^2}$; its area will be $\frac{2}{a^3} - \frac{2}{a^3 r^2} - \frac{2x}{a^2} - \frac{x^2}{a r^2}$.

I beg leave, in this place, to take notice, that in the theorem in line 12. page 63. of the second edition of my book of annuities on lives, instead of \mathcal{P} , it ought to be $\frac{p}{n} \mathcal{P}$; where n and p represent the complements of the age, in the beginning and end of a given interval of time.

And I desire the reader of that edition to adapt the fourth article of the rule put in words at length, in page 64, to the theorem so corrected: then the solution there given, and that in page 21. of the first edition, will perfectly agree; provided that the decrements of life be supposed, in both cases, uniform, from an age given, to the extremity of old-age.

I must also take notice of an accidental error, that has crept into the 25th proposition of the second edition; which I chuse to correct as follows;

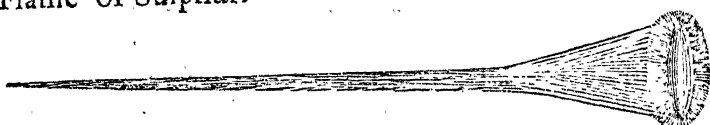
1. Let the first line of the proposition, and part of the second line, as far as A exclusive, be erased.

2. Let the solution proceed thus: since the life of A is supposed to be worth 14 years purchase, when interest is at 4 per cent. per annum, it follows, from

from our tables, that *A* must be 35 years of age; therefore find, by the twenty-third proposition, the value of an annuity of a life for 35 to continue for a limited time of 31 years: let that value be subtracted from the value of an annuity certain, to continue 31 years; and the remainder will be the value of the reversion.

XI. *The Appearance of a fiery Meteor, as seen by Mr. Cradock, communicated to the Royal Society by Mr. Henry Baker, F. R. S.*

Read June 7. 1744. **T**HE Head and Body emitted an extremely lucid and white Flame. The Tail appeared of a transparent Blue, like the Flame of Sulphur.



This *Phænomenon* was seen on *Sunday, May 27. 1744.* at 11 Minutes after 11 o' Clock at Night: its Direction from *S. E.* to *N. W.* or thereabouts; its Height seemingly not half a Mile.

It was seen, as here described, from the Terrace in *Somerſet-Gardens*, by me,

Zach. Cradock,
Of *Somerſet-Houſe.*

ERRATUM.

Page 48. for John Mackin, &c. read Martin Folkes, &c.

N. B. This Number 473. begins, by Mistake of the Printer, with *Page 1. Letter A.* instead of *Page 103. Letter O.* which is the Page and Letter following the preceding Number, which begins the *XLIII. Volume.*

Advertisement.

Mr. *Preston's* Map of ZETLAND, (*vulgo* Shetland) mention'd *p. 64.* is to be sold only at Mr. *Ames's* near the *Hermitage, Wappin.*

Printed for C. DAVIS, over-against *Gray's-Inn Gate*
in *Holbourn*, PRINTER to the ROYAL SOCIETY,
M.DCC.XLV.

Figure I. p. 34.

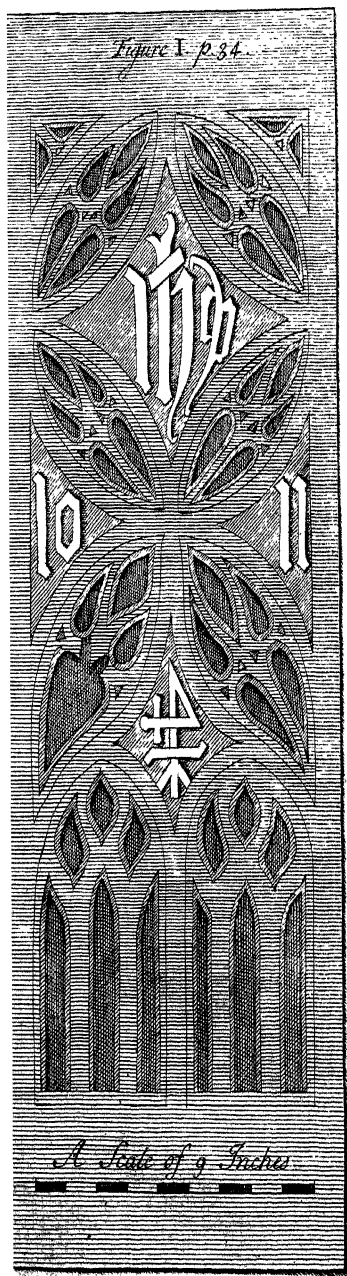
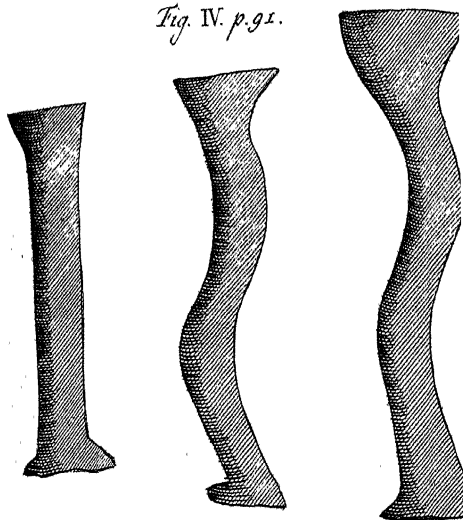


Fig. III. p. 91.

Fig. IV. p. 91.



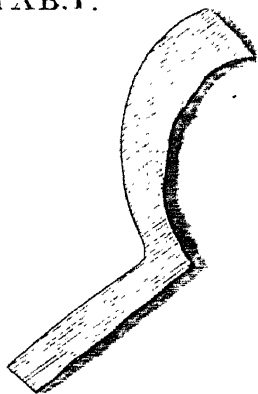
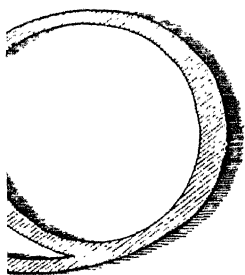
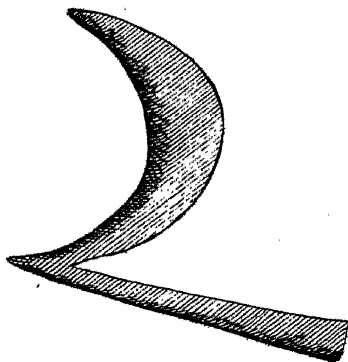


Fig. II.



Æ

p. 90.



PHILOSOPHICAL TRANSACTIONS.

For the Months of *June, July, August, September,
October, November, and December, 1744.*

The CONTENTS.

- I. *An Account of a Dissertation published in Latin by Dr. Weidler, F. R. S. in the Year 1727. concerning the vulgar Numeral Figures: As also some Remarks upon an Inscription, cut formerly in a Window belonging to the Parish Church of Rumsey in Hampshire: By John Ward, F. R. S. Rhet. Prof. Gresham.* p. 79.
- II. *A Letter from the Rev. Mr. Joseph Betts, M. A. and Fellow of University College, Oxon. to Martin Folkes, Esq; Pr. R. S. containing Observations on the late Comet, made at Sherborn and Oxford; with the Elements for computing its Motions.* p. 91.
- III. *Observatio de Scirrho Cerebelli, ab Alberto Hallero, R. S. S. Architect. Reg. & Med. Prof. Gotting. ad Reg. Soc. transmissa.* p. 100.
- IV. *An Essay upon the Causes of the different Colours of People in different Climates; by John Mitchell, M. D. Communicated to the Royal Society by Mr. Peter Collinson, F. R. S.* p. 102.
- V. *A Letter from William Hallett, M. D. to Henry Pemberton, M. D. F. R. S. & Chem. Prof. Gresham. containing the Case of a Lad, who was shot thro' the Lungs; drawn up by Mr. Nic. Peters junior, Surgeon.* p. 151.
- VI. *Extract of a Letter from Dr. John Bartram, to Mr. Peter Collinson, F. R. S. containing some Observations concerning the Salt-Marsh Muscle, the Oyster-Banks, and the Fresh-Water Muscle, of Pensilvania.* p. 157.
- VII. *A Letter from Mr. Christ. Hunter, to Dr. Mortimer, Secr. R. S. serving to accompany a Copy of an antient Roman Inscription at Rochester in Northumberland, and two others at Risingham.* p. 159.
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Printed for C. DAVIS, over-against *Gray's-Inn Gate* in *Holbourn*, PRINTER to the ROYAL SOCIETY, M.DCC.XLV.

ERRATUM in N° 473.

Page 65, l. 3. for 40", read 58".
N. B. The Clock was too slow 1 25".

- I. *An Account of a Dissertation published in Latin by Dr. Weidler, F. R. S. in the Year 1727, concerning the vulgar Numeral Figures: As also some Remarks upon an Inscription, cut formerly in a Window belonging to the Parish Church of Rumsey in Hampshire: By John Ward, F. R. S. Rhet. Prof. Gresham.*

Read at a Meeting of the Royal Society, June 7. 1744.

SOME Years since I had the Honour to lay before an Assembly of this Learned Body two Papers, concerning the Antiquity and Use of the *Arabian* or *Indian* Figures, and more especially in relation to *England*. And those Papers, being afterwards published in the *Philosophical Transactions* (a), occasioned the learned Dr. *Weidler*, Professor of the Mathematics at *Wittemberg*, and a Member of this Society, to transmit to Dr. *Mortimer* a *Dissertation* he had formerly printed upon that Subject (b). Which Discourse coming before the Society, they were pleased to refer it to my Perusal and Consideration; of which the following is a brief Account.

The Author begins his Discourse by observing the great Inconveniencies, that the Antients laboured under in their arithmetical Computations, which were usually made with the Letters of their several Languages, dif-

(a) Number 439.
(b) *De characteribus numerorum vulgaribus, eorum aetate, veterum monumentorum fide illustratis, Dissertatio mathematico-critica, a Joan. Frid. Weidlero, J. U. D. & Mathes. P. P. &c. Witemb. 1727. Quarto.*

ferently applied in different Countries. And he thinks it very strange, that, when it was always the Custom to distinguish their Numbers by *Decades*, they should not more early have fallen into the Method of using only ten different Characters, by means of which the largest Sums are now computed with so much Ease and Expedition. But the *Romans*, as he observes, had some Assistance from their *Abacus*, or *Counting Table*; a Description of which, with the Use of it, he has given from *Velfer*, by whom it was first published, and afterwards by *Gruter*, and others (a). And a Draught of the Table may be seen also in the *Philosophical Transactions*, Number 180. I would therefore only beg Leave to make a short Observation or two concerning it. And first I imagine, that the Θ , which is placed between the two Series of Rings on the Right-hand, may stand for the *Greek* Word *σπασμαλα*, *fractions*; as that Order of Rings denotes *Ounces* or *Parts* of the several following *Decades* towards the Left-hand, which are all *Asses* or *Integrals*. Nor was it unusual with the *Romans* to make use of single *Greek* Characters on some Occasions; as we use the *Latin* Letters, *l. s. d.* for *Pounds*, *Shillings*, and *Pence*. I would further remark, that the four Rings, placed by themselves on the Right-hand of the former, are doubtless Parts of the Ounce, as *Velfer* explained them. But, whereas *Peireskius* (as our Author observes from *Gassendus*) thought *Velfer* was mistaken in calling the

(a) *Grut. Inscrip. antiq.* ccxxiv. * *Pignor. De Servis*, p. 344.
ed. 1674. Octavo.

two lowermost of them *Duellas*, or *Thirds of an Ounce*; which he rather took to be *Sextulas*, or *Sixths*: I cannot but differ from both those Opinions, since they make this Order of Rings to disagree with the rest of the Table. For in each of the other Orders the several Rings, differently disposed, are suited to express any Number of Parts contained under it; and all of them together make one short of the Whole. Thus it is in the several *Decades*; and the Rings for Ounces may be so placed separately, as to express any Number under Eleven; and all of them united will make that Number, which falls short of the Ounce by one. But in these Parts of the Ounce, if the two undermost Rings are taken for *Thirds*, they will not apart express either the Number One or Two, nor by any Union the Number Five; and, if considered as *Sixths*, they will no way denote the Number One. And besides, in either Case, the whole Number together will exceed Eleven; that is, one short of the Parts, into which the Ounce was divided: which being an Integral to these, as the *As* was to the Ounce, such Parts of it were doubtless designed to be given here, as would correspond with the rest of the Table, in the Manner already explained. I apprehend therefore, that the two lowest Rings were intended for what *Volusius Maecianus* calls *dimidias Sextulas*, and *Duodecimas* (a); that is, the *Twelfths of an Ounce*; which, with the other two above them, will express any Part of the Ounce from One to Eleven, and so render the whole Table consistent with itself.

(a) See Gronov. *De Sestertiis*, p. 397.

But I return to our Author, who employs the remaining Part of his Discourse in treating of the Antiquity and Use of the *Arabian* or *Indian* Figures. And here he has given a very particular and accurate Account of the different Opinions of several Writers upon this Subject, but more especially of what *Kircher* and Dr. *Wallis* have said concerning it. The former of whom, as he observes, ventures to fix the precise Time, when the *Europeans* learned them of the *Arabians*; which was occasioned by the Assembly called together by *Alphonfus* King of *Castile*, for settling the *Astronomical Tables*, at which some *Moors* or *Arabians* were present. Now in those Tables, which were finished and published in the Year 1252, the Numbers are expressed in these Characters. *Kircher* thinks likewise, that the *Arabians* first borrowed them from the *Indians* about the Year 900; when, having subdued *Persia*, *Carmania*, and the Coast of *India*, they opened a Commerce with that Country. On the contrary, Dr. *Wallis*, as our Author remarks, has shewn, that these Figures were known to the *Europeans*, and used by them in Books of Astronomy and Arithmetic, long before the Time assigned by *Kircher*. But, as Dr. *Wallis* suspects, that the Characters found in some old Editions of *Boethius De Geometria*, very like the *Arabian* Figures, are different from the Original, or other antient Manuscripts of that Work; our Author acquaints us, that he himself saw in the public Library of the University at *Altorf* a Copy of it, which by the Form of the Letters appeared to him to have been written in the Eighth or Ninth Century; and that both the Shape and Situation of the numeral Characters were

were the same, as in the first Edition printed at *Venice* in 1492. He thinks therefore, that they might be the same, as in the Original of *Boethius*; and endeavours to shew, that they were then used in much the same Manner, as the *Arabian* Figures now are, in Sums of Multiplication and Division. And from thence he concludes, that such Characters must have been known in *Europe*, as early as the Beginning of the sixth Century; since *Boethius* was put to Death by *Theodoricus* King of the *Goths*, in the Year 524. As to the Objection, which may be made to this Opinion, from the Silence of Writers concerning it for several Ages after *Boethius*; he observes, that the same has happened in other Instances of a like Nature. Tho' he supposes, that both the Characters themselves, and the Use of them, was a Secret at that time, known only to Philosophers and Men of Learning, and not introduced into the common Affairs of Life; and that the first Invention of them was owing to the Eastern Nations, from whence they came to the *Greeks*, among whom the *Pythagoreans* were particularly remarkable for concealing their Knowledge from the Vulgar, and imparting it only to their Followers.

For the Illustration of his Discourse, the Author has prefixed to it a *Table* of numeral Characters, taken from Writers of different Ages and Countries; together with the *Helmdon Date*, in the Explication of which he follows Dr. *Wallis*: but he offers nothing further, so far as I could observe, in relation to the common Use of them, more early than what I have remarked in my former Papers. With this learned *Dissertation*, he transmitted likewise to
Dr.

Dr. *Mortimer* a small brass Quadrant, with the Numbers ingraven upon it in *Arabian* Figures, and the Date when it was made, namely, 1306. In this Quadrant all the Figures agree with those of *Johannes de Sacro Bosco*, except the 2; which in him is inverted thus, 2 (*a*), but on the Quadrant has the present Form.

I had long since delivered in this Paper; but that a reverend and learned Gentleman having communicated to the Society “An Account of an antient Date “ in *Arabian* Figures, upon the North Front of the “ Parish Church of *Rumsey* in *Hampshire*,” (*b*) I was desirous to get the best Information I could concerning it, in order to lay my Thoughts of it before them at the same time, to prevent the giving a double Trouble. But upon a very strict Inquiry I could not, for a great while, learn any thing further about it. And *Daniel Wray* Esquire, a worthy Member of this Society, was pleased to inform me, that being at *Rumsey* he had examined all the Parts of the Church carefully, but could find no such Date, nor any thing that resembled the Draught, which accompanied the Account. However, at length having by the Favour of the Reverend Mr. *Richard Newcome*, Rector of *Bishops Stoke* in that County, procured the Model of a Window, containing an Inscription not unlike that in the Draught, I now take leave to communicate the same, together with a Drawing of it, as in TAB. *Fig. 1.* It was found at *Rumsey*, where it served to stop up the Window of a Stable in an empty Inn, to which

(*a*) See *Philos. Trans.* n. 439.

(*b*) See *Philos. Trans.* n. 459. •
Place

Place it had probably been long before conveyed; since none of the Inhabitants remembered its being taken out of the Church, and nothing could then be discovered there, which bore the least Resemblance to it. Upon shewing it to a very skilful Architect, he immediately said, it was the Model of a Church Window; and that it was the Custom formerly to have such Models made for the Use of the Masons. Some time after I desired Mr. *Peter Newcome*, now a Member of this *Society*, to convey a Copy of the Draught above mention'd to his Brother, the Reverend Mr. *Benjamin Newcome*, who was then in that Neighbourhood, and from whom he soon received the following Account: " There was a Window in the North Front, that fell down thirty Years since, which, the Sexton tells me, he thinks something resembled the Draught (for I shewed it to him) as well as he can remember. That Window was bricked up for twenty three Years, but about seven Years ago was repaired and glazed." But, in the same Letter, speaking of the present State of it, he says: " I viewed the North Front, and could not see any Building or Window, either without or on the Inside, that in the least resembled the Draught." From the Sexton's Account therefore it seems to me not improbable, that there was formerly a Window somewhat in that Form, but larger, in the North Front, and that the Draught sent to the *Society* contains only Part of it; which being consider'd barely as a Date expressing the Year **1011**, the rest was not attended to. But, upon comparing it with the Model, I could not enter into that Sentiment; tho' I had not drawn up my Thoughts upon

upon it, when I first found it printed in the *Translations* (a). And I hope, what I now offer, will be esteemed only as an Inquiry after Truth; which in Things of this Nature can often rise no higher than Probability, wherein every one is at Liberty to determine, as he sees Cause.

I suppose therefore, that formerly there was a Window in the Church made after this Model; tho' the Place, where it stood, cannot now be determined, there having been forty Windows stopp'd up in that Church, as Mr. *Newcome* informs his Brother in another Letter. And more than one Window might be built at first by this Model, or with a little Variation from it. Wherefore the Characters being, as I apprehend, the same both in the Model and printed Draught, the same Interpretation may serve for both. Accordingly, I take the upper ones to stand for the Letters *ih̄s*, with a Stroke cross the Top of the *h̄*, being a Contraction of *ih̄esus*, as that Name was antiently written in *English*. And the Characters below these, which have been taken for Figures, when put together, make the Name *ion*, as it was likewise formerly written, without an *h̄*. Both which Words are so spelt in *Wiclif's English* Version of the *New Testament*, published by the Reverend Mr. *Lewis*. And doubtless, had the last Character been designed to express the Number *ii*, the two Strokes would have been kept separate, as they appear in the printed Draught; and not been joined both at the Top and Bottom, in the Form of the Letter *ii*, as

we find them in the Model. This Reading agrees very well, with what Mr. *Newcome* mentions in his first Letter; that he heard some Persons at *Rumsey* say, the Church had been dedicated to St. *John*. It seems therefore not improbable, that the Pictures both of *Christ* and St. *John* were formerly painted either in that Window, or near it. And this Opinion I am the more confirmed in from Mr. *Newcome's* Account in the same Letter, that there are three Crucifixes yet remaining about the Church, notwithstanding the many Windows now stopt up, and other great Alterations made in it at different times. One of these is painted on a Window behind the Communion Table, and represents *Christ* bearing his Cross. The second is on the South Wall, the Figure of which is five Feet three Inches high, and represents him in the usual Manner upon the Cross, with his Arms extended, and a Hand above pointing downward. It stands near the Ground, and appears very antient. The third, which is very rudely drawn, is on the Outside of the Church, near the Top of the West Front, in the same Posture as the second, accompanied with the six following Figures. On the Top of the Cross are two Angels. On the Sides are two other Figures in long Garments, designed probably to represent his Mother, and St. *John*, to whose Care he committed her, as we find recorded in his Gospel (a). Below are two Soldiers, one on the left Side holding up a Reed with a Sponge at the Top; and the other on the right piercing

his Side with a Spear, which latter Circumstance is likewise mentioned only in the Gospel of St. *John* (a). As the Model is cut out of an oaken Board, it may probably, notwithstanding its Thinness, be old enough to consist with the Time of that Spelling; but that it can be so antient as the Year 1011, some experienced Workmen, to whom I shewed it, think it wholly incredible.

The Explication here given may be further confirmed by the symbolical Figures underneath, which seem to be designed as an Emblem of the Trinity joined with the Cross. And it is well known, that St. *John* has treated more largely upon the Divinity of Christ, than any of the other Evangelists. *Plutarch* informs us, that *Xenocrates* the Philosopher resembled the Deity to an equilateral Triangle, the Genii to an *Isoceles*, and Men to a *Scalenum* (b). And a triangular Figure has been since applied by Christians to represent the Trinity, sometimes singly, and at other times with additional Lines expressing a Cross, as in this Model. So we find them variously combined upon the Medals of the Popes published by *Bonanni* (c). And nothing was more frequent formerly with Printers, than to place these complex Figures in the Front of their Books, at first doubtless

(a) Chap. xix. 34.

(b) Παράδειγμα δὲ τοῦ λόγου Ξενοκράτους μὲν ὁ Πλάτωνος ἑταῖρος ἐποιήσατο τὸ τῶν τετραγώνων, θεῶν μὲν ἐπεικάζας τὸ ἰσόπλευρον, θεῶν δὲ τὸ σκαληνόν, τὸ δὲ ἰσοσκελὲς δαιμονία· τὸ μὲν γὰρ ἴσον πάντι· τὸ δὲ ἄνισον πάντι· τὸ δὲ πᾶν μὲν ἴσον, πᾶν δ' ἄνισον, ὥσπερ ἡ δαιμόνων φύσις ἔχουσα καὶ πάθος θνητῶ καὶ θεῶν δύναμιν. *De defectu oraculorum*, edit. H. Steph. 1572. Vol. I. p. 740.

(c) *Numismata Pont. Rom. ed. Rom. 1699. fol.*

with a religious Intent, till at length by common Use, and being joined with other Devices, they became only Press-marks, and Badges of Distinction among the Trade; as they now are with Merchants, who mark their Goods with them both here and abroad.

A like Event has happened to another Character, made up of the *Greek* Letters X and P joined in this manner *P*, which we first meet with in some large brass * Coins of the *Ptolemeys*, Kings of *Egypt*, where it was placed on a civil Account. Some Writers have taken it for a Date, and others for the initial Letters of a proper Name (a). But as no Reasons are assigned for either of those Conjectures, I would rather suppose it an Abbreviation of the Word *XPHMA*, *Money*, impressed on those Pieces to denote their Currency as Money. Which might be thought proper, as they have not the Heads of the Kings stamped upon them, like their Silver and Gold Coins; but always that of *Jupiter* on the Front, and an Eagle perched on a Thunder-bolt upon the Reverse (b). And in that respect they seem to agree with such brass Medalions of the *Romans*, as we find stamp'd with the Letters S C; which in the Opinion of a judicious Antiquary were designed to intimate, that after they had been dispersed as Largeesses to the Populace, they were to pass for Money, like the common brass Coins marked with those Letters by the Authority of the Senate (c). An Impression of

(a) Pignorius *Epist.* 24.
χρημάτων.
ed. 1739.

(b) See Suidas in the *Word*
 (c) *La Science des Medailles*, Tom. I. pag. 211.

one of those *Greek* Coins above mentioned is here annexed (*a*), with which I was favoured by our honoured President. And it is well known, that the Monogram, which appears on it, was afterwards applied to a very different Purpose by the Emperor *Constantine* the Great, who made use of it to denote the Name *XPICTOC*, and placed it both on his Coins and military Ensigns; wherein he was followed not only by some of the succeeding Emperors, but also by private Persons, who out of Devotion put it on their Lamps and other Utensils (*b*). But afterwards it came to be used merely as a critical Note, to point out remarkable Passages in Manuscripts; as in later Times an Hand, with the Fore-finger extended, has been placed for the like Purpose in the Margin of printed Books. And then it stood for the initial Letters of the *Greek* Word *XPHCIMON*, *useful*, as we learn from *Isidore* (*c*). Other Instances might be produced of the like Nature, wherein Things have deviated from their original Use, and served different Purposes; but I forbear giving further Trouble to this Assembly on such minute Matters, and hope the Nature of the Subject will plead my Excuse, for what has been said already.

I shall only beg Leave to communicate the Copies of two antient Dates in *Arabian* Figures, which were imparted to me by the Reverend Dr. *William Warren*, Senior Fellow of *Trinity-Hall* in *Cambridge*. They were both taken by himself, and are of the same

(*a*) See TAB. Figure II. . (*b*) *Casalius De sacris Christian. ritibus*, p. 227. *Pignorius De Servis*, p. 32. ed. 1674. *Одъзво*. (*c*) *Orig. lib. 1. cap. 20.*

Size with the Originals. One is cut on a Beam running from the North East Corner of the Steeple to the School in the Church of *Ashford* in *Kent*, and expresses the Year 1295 (a). The other is cut in a Beam (over a great Passage) that is Part of a very old House at *Cambridge*, called the *Half Moon*, near *Magdalen-College*, and denotes the Year 1332 (b). The Figures of both are very rude, agreeable to those Times, being the oldest I have yet met with, except those at *Helmdon* (c).

II. *A Letter from the Rev. Mr. Joseph Betts, M. A. and Fellow of University College, Oxon. to Martin Folkes, Esq; Pr. R. S. containing Observations on the late Comet, made at Sherborn and Oxford; with the Elements for computing its Motions.*

Read June 14.
1744.

THE Comet which appeared towards the End of last *December*, and in the following Months *January* and *February*, 1744. was first seen in *England*, at the Observatory of the Right Honourable the Earl of *Macclesfield*, *Dec. 23.* between 5 and 6 o' Clock in the Evening. It formed, at that time, an obtuse-angled Triangle, with (α) of *Andromeda*, and (γ) *Pegasi*, the Comet being at the

(a) See TAB. Figure III. (b) See TAB. Figure IV. (c) See Philos. Transact. n. 439.

obtuse Angle ; and its Passage over the Meridian was observed at $5^h 32'$, mean *Oxford* Time. His Lordship's Observer could not then take its Distance from the *Vertex* accurately, the Comet's *Transit* being unexpected ; however, by an Observation made at *Paris* the same Evening by Mr. *Monnier* we have the Distance very nearly.

His Lordship the next Day acquainting the Reverend Mr. Professor *Bliss* with this Discovery, gave us an Opportunity of looking after it at *Oxford* ; but, unfortunately, bad Weather, and a continued Succession of cloudy Evenings prevented our observing it, till *Dec. 31.* but the Weather proving more favourable at *Sherborn*, its right Ascensions and Declinations were taken by his Lordship, the Result of which Observations is as follows :

Note, That the equal Time is made use of in the following Observations, and that the Comet's *Transits* (reduced to the Meridian of *Oxford*) are only given to the nearest Half-Minute, as being sufficient for computing its Places.

Dec. 23^d 5^h 32ⁿ) The right Ascension of the Comet by the *Transit* Instrument at *Sherborn* was found to be $5^{\circ} 48' 1''$; and its Polar Distance by Mr. *Monnier* $68^{\circ} 18' 35''$.

Dec. 27^d 5^h 7¹/₂) The right Ascension of the Comet, observed at *Sherborn*, was $3^{\circ} 41' 7''$; and its Declination $21^{\circ} 7' 13''$ North.

Dec. 28^d 5^h 1¹/₂) The observed right Ascension of the Comet was $3^{\circ} 11' 8''$; and its Distance from the Pole $69^{\circ} 0' 38''$.

Dec.

Dec. 31^d 4^h 44') The right Ascension of the Comet, by the *Transit* Instrument, was found to be $1^{\circ} 44' 40''$; and its Declination $20^{\circ} 36' 37''$ North.

The same Evening, at $5^h 53'$) The Sky favouring us at *Oxford*, the Distance of the Comet from *Aldebaran*, taken with *Hadley's* Quadrant, was $60^{\circ} 10'$, corrected for Refraction, $60^{\circ} 11'$; from (γ) *Pegasi* $7^{\circ} 2\frac{1}{2}'$; corrected, $7^{\circ} 2' 40''$.

January 12^d 9^h 10') The Comet followed (ϕ) *Pegasi*, in a five Foot Glass, $1^{\circ} 43' 32''$ of right Ascension; and was more northerly than the Star $1^{\circ} 36' 00''$: The right Ascension of the Star, by the *Greenwich* Observations at that time, was $354^{\circ} 52' 12''$, its Declination $17^{\circ} 41' 55''$: Therefore the Comet's right Ascension was $356^{\circ} 35' 44''$, and its Declination $19^{\circ} 17' 55''$.

January 13^d 6^h 30') The Distance of the Comet from *Aldebaran*, at a *Medium* of several Trials by the Quadrant, was $65^{\circ} 26' 50''$; corrected for Refraction $65^{\circ} 28' 10''$; its Distance from (γ) *Pegasi* $6^{\circ} 31\frac{1}{2}'$; corrected, $6^{\circ} 31' 45''$.

At $8^h 20'$) The Comet followed (ϕ) *Pegasi* $1^{\circ} 21' 13''$ of right Ascension; and was more northerly than the Star $1^{\circ} 30' 33''$. Hence the Comet's right Ascension was $356^{\circ} 13' 25''$; and its Declination $19^{\circ} 12' 28''$ North.

January 16^d at 6^h 33') The Comet's Distance was observed by the Quadrant from *Aldebaran* $66^{\circ} 36\frac{1}{4}'$; corrected for Refraction $66^{\circ} 38' 10''$; from (γ) *Pegasi* $7^{\circ} 0\frac{3}{4}'$; corrected $7^{\circ} 1'$.

At 8^h the same Evening) The Comet followed (ϕ) *Pegasi* in the five Foot Glass $10' 24''$ of right Ascension; and was more northerly than the
Star

Star $1^{\circ} 13' 24''$. Hence the Comet's right Ascension was $355^{\circ} 2' 36''$; and its Declination $18^{\circ} 55' 19''$ North.

January 23^d 6^h 11') The Comet's Distance was observed by the Quadrant from *Aldebaran* $69^{\circ} 26' \frac{2}{3}$; corrected for Refraction $69^{\circ} 28' 5''$; from (γ) *Pegasi* $8^{\circ} 42' \frac{1}{4}$; corrected $8^{\circ} 42' 35''$.

January 23^d 7^h 29') The Comet preceded (ϕ) *Pegasi* $2^{\circ} 43' 27''$ in right Ascension; and was North of the Star, in the 8 Foot Glass, $26' 32''$. Hence the Comet's right Ascension was $352^{\circ} 8' 46''$; and its Declination $18^{\circ} 8' 27''$.

The Comet this Evening appeared exceedingly bright and distinct, and the Diameter of its *Nucleus* nearly equal to that of *Jupiter's*; its Tail, extending above 16 Degrees from its Body, pointed towards (ζ) of *Andromeda*; and was in Length (supposing the Sun's Parallax $10''$) above 23 Millions of Miles; but cloudy Weather succeeding, we lost this agreeable Sight till Feb. 5.

Feb. 5th 7^h 31' $\frac{1}{2}$) A small Star of *Pegasus*, marked (α) by *Bayer*, preceded the Comet in right Ascension $1^{\circ} 40' 20''$; and was South of the Star $54' 23''$. The right Ascension of the Star, by the *Greenwich* Observations at that time, was $343^{\circ} 0' 4''$; its Declination $13^{\circ} 49' 56''$: Wherefore the Comet's right Ascension was $344^{\circ} 40' 24''$; and its Declination $14^{\circ} 44' 19''$ North.

Feb. 11th 6^h 37' $\frac{1}{2}$) The Comet followed (ξ) *Pegasi*; the Correction for Refraction being allowed $43' 1''$ in right Ascension; and was South of the Star $50' 3''$: The right Ascension of (ξ), by the *Greenwich* Observations at that time, was $338^{\circ} 28' 24''$; its

its Declination $10^{\circ} 51' 3''$: Therefore the Comet's right Ascension was $339^{\circ} 11' 25''$; and its Declination $10^{\circ} 1'$ North.

Feb. 12^d 6^h 33') The Comet followed (ζ) *Pegasi* $56' 45''$ of right Ascension; and was more southerly than the Star $44' 42''$. The right Ascension of (ζ), by the *Greenwich* Observations at that time, was $337^{\circ} 10' 15''$; its polar Distance $80^{\circ} 29' 53''$. Hence the Comet's right Ascension was $338^{\circ} 7' .00''$; and its Declination $8^{\circ} 45' 25''$ North.

Feb. 13^d 6^h 25') The Comet preceded (ρ) *Pegasi* $7^{\circ} 41' 31''$ in right Ascension; and was more southerly than the Star $1' 13''$: The right Ascension of the Star, at that time, was $344^{\circ} 41' 55''$; its Polar Distance $82^{\circ} 40'$: Whence the right Ascension of the Comet was $337^{\circ} 0' 24''$; and its Declination $7^{\circ} 18' 47''$ North.

This was the last Observation made at *Oxford*; the Comet being now so near the Sun, and withal so low in the Evening, that the great Difficulty of finding any Star to compare it with, made us desist from attempting it again; however, the prodigious Brightness it acquired, by its near Approach to the Sun, made it visible in the Day-time. And at *Sherborn*,

Feb. 16^d 23^h 42' $\frac{1}{2}$) Its right Ascension, by the *Transit* Instrument, was found to be $333^{\circ} 13' 53''$; and its Declination $0^{\circ} 2' 40''$ South.

Feb. 17^d 23^h 36') The right Ascension was observed $332^{\circ} 33' 20''$; and its Declination $2^{\circ} 29' 00''$.

By the Help of these Observations, which were made by the Rev. Mr. Professor *Bliss* (the *Transits* excepted taken at *Sherborn*), I was enabled, by the Method delivered in the third Book of the

Principia, to determine the Comet's Parabolic Trajectory; and found the Place of the ascending Node to be in $8. 15^{\circ} 45' 20''$; The Logarithm of the *Perihelion* Distance 9,346472: The Logarithm of the diurnal Motion 0,940420: The Place of the *Perihelion* $\approx 17^{\circ} 12' 55''$; the Distance of the *Perihelion* from the Node $151^{\circ} 27' 35''$: The Logarithm, Sine, and Co-sine of the Inclination of the Orbit to the Ecliptic 9,865138, 9,832616: And thence the Time the Comet was in the *Vertex* of the *Parabola*, or the Time of the *Perihelion*, Feb. 19^d 8^h 12': The Motion of the Comet, in its Orbit thus situated, was direct, or according to the Order of the Signs.

From these Elements, by the Help of Dr. *Halley's* general Table (to which they are adapted), I computed the Comet's Places for the Times of Observation, exhibited in the following Table: To which are added the Comet's Longitudes and Latitudes deduced from the observed right Ascensions and Declinations; together with the Errors between the observed and computed Places; the Observations being all reduced to *Oxford* mean Time.

Equal Time at <i>Oxford.</i>	Longit. Comet observed.	North Latit. observed.	Longit. Comet computed.	North Latit. computed.	Diff. in Long.	Diff. in Latit.
D H ' "	o ' " "	o ' " "	o ' " "	o ' " "	" "	" "
<i>Dec.</i> 23 5 32	γ . 14 10 2	17 33 11	γ . 14 10 3	17 33 37	1—	26—
27 5 7 $\frac{1}{2}$	γ . 12 2 25	17 51 29	γ . 12 2 26	17 51 47	1—	18—
28 5 1 $\frac{1}{2}$	γ . 11 32 11	17 55 54	γ . 11 32 14	17 56 8	3—	14—
31 4 44	γ . 10 4 57	18 9 3	γ . 10 5 16	18 8 53	19—	10+
5 53	γ . 10 4 11	18 9 37	γ . 10 3 55	18 9 6	16+	31+
<i>Jan.</i> 12 9 10	γ . 4 52 5	18 59 37	γ . 4 52 24	18 59 13	19—	24+
13 6 20	γ . 4 31 40	19 2 31	γ . 4 31 13	19 2 49	27+	18—
8 20	γ . 4 29 27	19 3 32	γ . 4 26 6	19 3 12	21+	20+
16 6 33	γ . 3 18 43	19 15 47	γ . 3 18 27	19 15 13	16+	34+
8 00	γ . 3 17 31	19 16 7	γ . 3 17 00	19 15 30	31+	37+
6 11	γ . 0 19 45	19 42 30	γ . 0 19 16	19 42 1	29+	29+
23 7 29	γ . 0 17 58	19 42 47	γ . 0 17 45	19 42 12	13+	35+
<i>Feb.</i> 5 7 31 $\frac{1}{2}$	κ . 21 52 37	19 35 00	κ . 21 52 56	19 34 42	19—	18+
11 6 37 $\frac{1}{2}$	κ . 14 42 45	17 23 30	κ . 14 42 58	17 24 5	13—	35—
12 6 33	κ . 13 10 36	16 38 40	κ . 13 10 52	16 39 17	16—	37—
6 25	κ . 11 32 50	15 43 45	κ . 11 33 16	15 44 16	26—	31—
13 23 41 $\frac{1}{2}$	κ . 5 9 14	10 17 40	κ . 5 9 1	10 18 8	13+	28—
17 23 35	κ . 3 37 37	8 15 39	κ . 3 37 11	8 16 3	20+	24—

Perhaps it may not be thought foreign to my Purpose to remark, that the Nodes of the Comet, and the Planet *Mercury*, are situated within less than half a Degree of each other; which, I suppose, gave Rise to a Report, that the Comet had carried *Mercury* from its Orbit. In order therefore to find how nearly they approached each other, I had the Curiosity to bring the Matter to Calculation; and presently found, there was above a Week's Difference in the Times of their Coming to the Nodes; the Comet passing its descending Node, *Feb.* 22. about 2^h in the Morning; and *Mercury* not coming to his till *Feb.* 29. the Comet moving all that Time Southwards with a prodigious Velocity. Again, computing their Helio-centric Conjunction, which happened *Feb.* 18. about 1^h in the Afternoon, I found the Comet was, at that time, distant from *Mercury* nearly $\frac{1}{2}$ Part of the Semi-diameter of the *Orbis magnus*; being almost twice as near to the Sun as the Planet φ ; and having then 31° 30' of North Latitude; *Mercury's* not exceeding 3° 58' (to an Eye in the Sun): Whence it is easily collected, that the Comet could have no sensible Influence upon φ 's Motion.

I shall now only beg Leave to observe, that the Elements above-given cannot possibly differ much from the true. For, after an Interval of Two Months (in which time the Comet had gone thro' almost $\frac{1}{3}$ Part of its Orbit), it is surprising to find the observed and computed Places agree so accurately, that the Difference no-where amounts to a Minute. In some Parts of the Orbit, the Agreement is still greater; particularly, in the Observations made at *Sherborn*, which come within half that Quantity; and would have

have corresponded still nearer, but that I was ambitious to confine the whole Series of Observations within the narrow Limit above-mentioned; which I have at last compassed, not without a long and tedious Calculation: But, long and tedious as it was, I shall not repent of the Trouble I have been at, if I find my Endeavours agreeable to my astronomical Reader.

It may, perhaps, be expected (considering the great Part of its Orbit the Comet described during its Appearance), that I should have settled its Period, and foretold its Return. --- This, I confess, would have given me great Pleasure; neither would I have spared any Pains in the Inquiry, had I met with any Prospect of Success; but the Period, upon my attempting it at first, came out so prodigiously long (the transverse Ax of the Ellipse being nearly equal to Infinity), that I was stopp'd short in my Inquiry; neither could I prevail upon myself to resume the Subject again, when, upon turning over *Hevelius*, I found the Account of Comets, which had appeared at long Intervals of Time from us (as it might reasonably be expected) so short and uncertain: But, could I procure *Celsius's* Observations, or any made after the *Perihelion*, I might be induced to fall to Work again; and would not fail communicating the Result, did I meet with Success; and, at the same time, the Elements of the Comet, which appeared in 1742, which I have had by me some time; not so perfect as I could wish, but as perfect as may be obtained from the few Observations I met with.

The Comet was in Conjunction with the Sun, *Feb.* 15. about Midnight; and its Perigee, *Feb.* 16. about

about 1^h in the Afternoon ; at which time it was somewhat nearer the Earth than the Sun is at its Perigee ; the Comet's Distance being then (,83) and the Sun's (,98) such Parts, as the Semidiameter of the *Magnus Orbis* is (,100) ; from which we may have some Idea of the Comet's Magnitude ; and therefore may suppose it, at least, equal to the Earth.

Joseph Betts.

III. *Observatio de Scirrho Cerebelli, ab Alberto Hallero, R. S. S. Archiat. Reg. & Med. Prof. Gotting. ad Reg. Soc. transmissa.*

Read June 14.
1744.

Cerebellum sedem principii vitalis fecit *Willisus* ; et, qui hanc hypothesein ornatissime proposuit, *Boerhaavius*. Verum, præter plurima, quæ contra nervorum vitalium distinctam classim faciunt, et præter experimenta capta à cl. *Lapeyronie*, per quæ constat vulnera cerebelli inflicta canibus, non continuo lethalia fuisse, hanc etiam hypothesein infirmant morbi cerebelli, satis rari, sed aliqui tamen, etiam maximi, et qui cerebellum pene inutile redderent, qui ætatem tamen tulerunt. Manifesta certe inde nascitur suspicio, non adeo, ut *Willisiana* requireret hypothesis, proximam vitalium cordis nervorum, in cerebello & solo, originem esse. Quos præterea satis constat, minima parte à cerebri nervi octavi, et intercostalis ramis, sed à spinalibus imprimis provenire, ex quibus, utrumque ganglion cervicale, multo magis, quam à cerebri propagine, provenit.

Ergo

Ergo memoria digna visa est nuperrima in puella mendica observatio. Adlata est misella in theatrum die 21. *Januarii* 1744. cum ante annos fere sex nata fuisse videretur. Multi passim in emaciato corpufculo scirrhi, glandularum mesentericarum, inguinalium, bronchialium.

Cum historiam carotidis externæ, et rami maxime, quem maxillarem internum vocat clar. *Winslowus*, persequeretur, in cerebello miri mali mihi apparuit effigies. Lobus ejus sinister pene totus, pertinaciter duræ matri occiput vestienti adhærebat. Incidi tentorium; reperi scirrhum enormem, duarum utrinque unciarum diametro, in quem degeneraverat tota aut medullaris, aut corticalis cerebelli substantia. Uniformiter spissus erat tumor, fibrosus, renis fere ad modum, et scissilis, nullo vase, quæ penitissime repleta erant, in sectione adparente, nulla superstite nota aut cinerei corticis, aut arbuscularum medullarium.

Quæcunque fuerit tanti mali origo, adparet cerebelli mediam partem in hac puella, neque brevi tempore, inutilem fuisse; et vicitavit tamen misera, stipemque ostiatim petiit.

Unica parallela scirrhi cerebelli historia, in stupido puero reperti, legitur in *Actis Parisinis*, 1705. n. 13.

IV. *An Essay upon the Causes of the different Colours of People in different Climates*; by John Mitchell, M. D. Communicated to the Royal Society by Mr. Peter Collinson, F. R. S. *

Read at several Meetings, from May 3, to June 14. 1744.

THE Cause of the Colour of Negroes being a Subject so little known, but so much inquired after, and withal so curious and useful, as to excite the particular Attention and

* *Extract of Part of Dr. Mitchell's Letter to P. Collinson, F. R. S. from Urbana in Virginia, April 12. 1743.*

S I R,

I Promised you some Returns for your Favours, by sending you my Essay on that strange *Phænomenon* in Nature, the Cause of the Colour of Negroes.

I cannot promise you much, because my daily Employments engross so much of my Time: But this I shall beg Leave to say, that with great Care I have made the Experiments and Observations on purpose to find out the Truth. I must own I was surpris'd at first to see them differ from the Opinions of some learned Men; especially in Matter of Fact; which they rather allege than prove, relating to the fluid *Mucus* of the *Cuticula*, or *Corpus reticulare*; for which Reason I repeated my Experiments on living Subjects several times, but could never see any Tokens of that black Juice.

I am apt to think, that the Anatomists, perceiving the *Corpus reticulare* in dead Bodies to be a soft pappy Substance, have rather imagin'd than really found it to be, or contain, a fluid mucous Humour.

But, as I am afraid this will come too late for a Solution of the Prize-Problem, propos'd by the Academy of *Bourdeaux*. If you think it deserves so great an Honour, pray communicate it to the *Royal Society*; and if it merits their particular Regard, I submit it to be printed in your learned and curious Memoirs. I am

Your obliged humble Servant,

John Mitchell.

and Inquiries of the learned in *Europe*, particularly the Academy of *Bourdeaux*, in their Prize-Problems, to which they have received no satisfactory Answer, as I have been lately informed; I have therefore ventured to offer my Thoughts to you on that Subject, having had frequent Opportunities to make the proper and necessary Observations; and which, at least, I hope, may not be unacceptable, that some one, of better Skill, and more Leisure, for such nice philosophical Inquiries, may make more correct and fuller Conclusions from them. This Problem supposes the Knowledge of the Causes of Colours in general; so that if I can deduce the Colour of the Skin from its Structure, &c. in the same manner, and for the same Reasons, from which the great *Newton* deduces the Colours of other Substances, it is all I can pretend to, which will be as much as that Branch of Philosophy will permit: And as this Problem will include the Cause of the Colour of the Skin in general, I shall first inquire into the Cause of the Colour of white People; with a Change from that Colour in some preternatural Affections, whose Causes seem not well understood. This I shall do in so many Propositions, that you may the better perceive how far each Proposition is demonstrated, or of what Consequence it may be in deducing the desired Solution of the Problem, concerning the Cause of the Colour of Negroes.

PROPOSITION I.

The Colour of white People proceeds from the Colour which the Epidermis transmits; that is, from the Colour of the Parts under the Epidermis, rather than from any Colour of its own.

The Truth of this Proposition will plainly appear to those, who consider, that the Colour of white People is always more or less clear or vivid, as the Skin is thinner or thicker, finer or coarser; that is, as it is more or less adapted to transmit the Colour of the white Parts below it. These Parts are the *Parenchyma* of the Skin, *Corpus reticulare*, *Papillæ nervosæ*, the limpid and clear Juices contained in the Vessels, and perhaps the inner *Epidermis* itself may appear thro' its outer porous Coverlet; all which Parts we know are white, and are what appear so in white People.

But this will be better confirmed, from the following Considerations; 1. The Palms of the Hands, Lips, &c. where the *Epidermis* and Skin are so thin, as to transmit the Colour from any thing below them, appear red, or of the Colour of the red Blood under them; especially in those in whom the Skin is fine and thin; but where the Skin is thick and coarse, those Parts appear almost of the same Colour with the rest of the Body. 2. The Blushings of the Cheeks, and their Redness in Fevers, seem to be another Proof of this Cause of their Colour; for, in a Moment, they change from a pale to a deep Red; but no one will imagine, that the *Epidermis* then changes its Colour, or Power of reflecting the Rays of Light; but

but that it transmits the Colour of the Blood; which is, at such times, more forcibly driven into the capillary subcutaneous Vessels, and shines thro' the *Epidermis*; but, before, these Vessels contained only a serous Liquor, and, accordingly, the Skin appeared of that Colour: Which will further appear upon squeezing such red Parts, which drives the Blood out of them, and makes them appear white; whereas, on removing such Pressure, they recover their Colour, as the Blood does its Place. 3. The yellow Colour of the Skin in the Jaundice is a further Proof of this Assertion; where the yellow Bile is diffused thro' the Vessels of the *Cutis*, and appears thro' the *Epidermis*; but no one will imagine, that the *Epidermis* itself receives this viscid Bile into its Vessels; which are so small, that many accurate Anatomists, as *Morgagni*, have denied it to have any Vessels at all; and the most accurate could never shew them. 4. The pale Look of those, in whom the Blood is viscid, or circulates with little Force, shews, that the *Epidermis* then transmits the Colour of the Juices and Fibres below it, which are then unmix'd with red Blood. 5. The same is manifest in those whose Blood is poor and serous, as the Leucophlegmatic, &c. in whom the *Epidermis* transmits the Colour of the Water or Serum under it.

From hence it appears, that the *Epidermis* is a transparent Membrane, which easily shews the Colour of the Parts under it, in the same manner as the *Cornea* of the Eye transmits the Colour of the *Iris*. But this will appear more plainly, from some Considerations below; where we shall assign the Cause of this Pellucidity; and shew, that the Number of

Pores in the *Epidermis* necessarily make it transparent; and that the Smallness of the Particles, into which it is divided by them, make it unfit to reflect any Rays of Light, and consequently to manifest any Colour of its own.

But, to this, some, perhaps, may object, that the *Epidermis*, when taken off of the Body, appears white, and consequently reflects such Rays of Light. But then we must consider, that its Pores and Fibres are much contracted, and its Substance, consequently, rendered more dense, and fitter to reflect any Colour; besides, it is then evacuated of those transparent limpid Juices, which it before contained, from the Vessels of the Skin which pervade it; and which, as Sir *Isaac Newton* shews (a), will render any Body transparent; especially since those Juices, which then pervade the *Epidermis*, are nighly of the same Density with it; since all Accretion and Nutrition is from such. Accordingly we see, that when the *Epidermis* is taken off of the Body, it appears transparent enough for what we have assigned, especially its external *Lamina*. This may be further perceived, by holding the Hands of some Persons of thin Skins, and much shrivel'd, in a certain Light; when you may perceive the Colour which this Membrane reflects, which is of a silver White, like all other pellucid *Lamelle*; very different from the Colour which it transmits from the Parts under it; of which Colour likewise the Scales of the *Epidermis* appear, when rubbed off on black Cloth, or when scaled off in a

(a) Opt. l. 2. Par. 3. Prop. III.

Leptosis; a remarkable Instance of which Dr. *Turner* affords us (a): Altho' it cannot be denied, but that the Cuticle may reflect some small Portion of the Rays of Light; which, however, seem to have but little Share in occasioning the Colour of the Body, to what those Colours have, which are transmitted thro' it, from the other less diaphanous Membranes under it.

PROPOSITION II.

The Skins of Negroes are of a thicker Substance, and denser Texture, than those of white People, and transmit no Colour thro' them.

For the Truth of the first Part of this Proposition, we need only appeal to our Senses, and examine the Skins of Negroes when separated from the Body; when not only the *Cutis*, but even the *Epidermis*, will appear to be much thicker and tougher, *ceteris paribus*, than in white People. But, because the Substance and Texture, especially of the *Epidermis*, is not a little alter'd in anatomical Preparations, and that in such a Measure as to alter the Texture perhaps, on which the Colour depends, by Boiling, Soaking, Peeling, &c. let us examine the Skins of Negroes on their Body; where they will appear, from the following Considerations, to have all the Properties assigned: 1. In Bleeding, or otherwise Cutting their Skins, they feel more tough and thick, than in white People. 2. When the *Epidermis* is sepa-

(a) *De Morb. Cutan.* p. m. IV.

rated by *Cantharides*, or Fire, it is much tougher and thicker, *ceteris paribus*, and more difficult to raise, in black than white People. 3. Negroes are never subject to be sun-burnt, or have their Skins blistered by any such Degree of Heat, as Whites are; but, if we consider, that a black Body retains more Heat than a white one, or any other Colour, it will be very plain, that their Skins must be thicker or denser, *i. e.* more cartilaginous or callous, to ward off this Violence of the Sun's Beams. 4. Altho' their Skins, in some particular Subjects, should not be so very thick in Substance, yet in Winter, when they are dry, and not covered with that greasy Sweat which transudes thro' them in Summer, their Skins feel more coarse, hard, and rigid; as they do in ardent Fevers, with a dry Skin. 5. Their Exemption from some cutaneous Diseases, as the Itch, prickly Heat or *Effere*, which no adult Negroes are troubled with, but those of fine and thin Skins are most subject to, shew the Thickness or Callosity of their Skins, which are not easily affected from slight Causes. 6. But not only the Thickness, but likewise the Opacity of their Skins, will appear, from their never looking red in Blushing, or ardent Fevers, with internal Inflammations, nor in the Measles, or Small pox; where, altho' the Blood must be forcibly impelled into the subcutaneous Vessels, yet it does not appear thro' the *Epidermis*. The like may be said of their Veins; which, altho' large and shallow, yet do not appear ^{like} till the Skin is cut. 7. In the Jaundice, *Anasarca*, &c. the Skin of Negroes never shews the Colour of the Parts under it; altho' visible enough in the Eyes: Of which I lately saw a more convincing

Proof

Proof in some Negroes labouring under a bilious Fever, in whom the *Serum* of the Blood, when let, was of a deep bilious Yellow, but no yellow Colour appeared on the Skin, altho' plain enough to be seen in the Eyes.

Corollary. From hence might be deduced one plain Cause of the Blackness of Negroes; for, if the Colour of the Skin depends on what it transmits, and the Skins of Negroes transmit no Colour thro' them, they must, for that Reason, appear black; according to the known Doctrine of Light and Colours, That, where-ever there is a Privation of Light or Colour, there, of course, ensues Darkness or Blackness. But, as most solid Bodies, which are not pellucid, do generally reflect some Colour, which we know no black Body does, we shall next inquire into the particular Make of their Skins, by which they are rendered incapable to reflect, as well as to transmit, the Rays of Light.

PROPOSITION III.

The Part of the Skin which appears black in Negroes, is the Corpus reticulare Cutis. and external Lamella of the Epidermis: And all other Parts are of the same Colour in Them with those of white People, except the Fibres which pass between those Two Parts.

For a Proof of this Proposition, we must examine the Structure of the Skins of Negroes more narrowly, which may be done after Blistering with *Cantharides*, or after a Scald or Burn; when their Skins have appeared to me in the following Manner: The Cuticle,

cle, which is separated, appears nighly of the same Colour on the Outside, as before such Separation from the Body; but, on the inner Side, is almost as white as the same Part in white People. This Cuticle is almost always, in Blistering with *Cantharides*, divided into two *Lamellæ*; especially on the Thighs, where it is as thick almost as both the Skin and Scarf-skin of white People: The Surfaces, by which these two Parts or *Lamellæ* of the *Epidermis* cohere, are partly white, and partly black; for you may see many black Fibres pervading the inner *Lamella*, and perforating the upper one, which appear like so many black Spots on these two Surfaces, when separated from one another; but these black Spots do not appear on the inner Surface of this inner *Lamella*; these Fibres being, as it were, contracted within the two *Lamellæ*, upon the external Surface of this inner one. The inner Surface of the outer *Lamella* of the *Epidermis*, or at least of the outermost of the two into which it is divided by *Cantharides*, appears to be a whitish Membrane, like the other Membranes of the human Body; except the foremention'd black Spots, which appear on this likewise, and the Colour it receives from its external black Surface, which appears, in some measure, thro' the inner Surface, and makes the Whiteness on it appear very superficial. This outer *Lamella* is thicker and tougher, and not so pellucid, as in Whites.

By scraping these *Lamellæ* of the Cuticle of Negroes, they may be made more white, and these black Spots scraped off, by which the under *Lamella* will become as white as any Membrane almost of white People; and several white *Striæ* may be scraped

scraped off from the outer *Lamellæ*, by which both its Surfaces will become more nighly of the same black Colour: From whence the Cuticle would appear to consist of, or be composed of, many different *Lamellæ*, and those of different Colours, so that the external one only is black; which Blackness is easily scraped off from the Membranes, by any thing that will abrade the *Fibrillæ*; but it is not to be removed by only soaking or wringing out the Cuticle in any common *Menstruum*, which might dissolve and extract any Juices in it; from whence, by the by, this Blackness would appear to proceed from these *Fibrillæ* and Scales, and not from any Juices. But, as these minute nervous *Fibrillæ* are distributed thro' and all over the other coarser Membranes, so they very easily render them black, by insinuating themselves into their Interstices (a).

Under the *Epidermis* of Negroes, when separated in a living Subject, by Blistering, appears, as it were, a third Membrane between That and the *Cutis vera*: This is the *Corpus reticulare* MALPIGHII, which differs from the same Part in white People in two Respects; for, in Negroes, it is of a black Colour all over the Body, where they appear black; and whereas, in white People, it is of a soft, pappy, or mucous Substance, and can hardly be separated but in pappy Flakes, in Blacks it is separated very often, by the Force of Epispastics, from both Skin and Cuticle, and may often be peeled off, like a Membrane, from the *Cutis*, as the *Epidermis* is from it; whilst in other

(a) Vide Newton. Opt. p. 222, Edit. Latin.

Places, by a less Force of the Epispastic, it is closely adhering to the *Cutis*, as the *Epidermis* itself often is: This membranous Expansion is of a much thicker Substance, or denser Texture, than the same Part in Whites; and from this seem to proceed the black Fibres, which pervade the *Epidermis*, and end in its external Surface.

The *Cutis* itself, which lies under this black membranous Expansion, and to which it is closely connected, is of a white Colour in Negroes, somewhat like the Skin of many brown-skin'd white People; but when this black *Corpus reticulare* is upon it, after the *Epidermis* is separated, they appear, when both connected together, of a brown Copper-Colour, somewhat like the Colour of an *Indian* or *Molatto*; some of the Colour of the white Skin below being transmitted thro' this thin black Membrane: Which seems to shew, in what manner the Colour of these *Indians* and *Molattoes* may be occasion'd, by the Colour of the white Membranes under their Cuticles appearing partly and imperfectly thro' them, as the white Skin does thro' this *Corpus reticulare*.

From hence, the Formation of the *Epidermis* seems to be more easily shewn, and more completely deduced, than from any Preparation of it in white People. For the external *Lamella* of it manifestly arises from the *Corpus reticulare*, by the Intervention of the black *Fibrillæ*, which we have shewn to pervade the inner *Lamellæ* of the *Epidermis*; and this *Corpus reticulare* itself arises from the subcutaneous Nerves, so nicely and accurately delineated by *Eustachius*;

Eustachius (a): Each of the Fibres from this *Corpus reticulare* seem to form, or be expanded into, a small Scale, at their Termination on the outer Surface of the Cuticle, in the same manner as other Vessels of the Body, where not inserted into any Part, terminate in a membranous *Aponeurosis*. But these do not seem to be the only Parts of the *Epidermis*, as the white *Lamellæ* of it are plainly different from the black; for as this black *Lamella* is an Expansion of the nervous *Plexus*, which proceeds from the Nerves of the Skin, so it is very probable, that each Series of exhaling and inhaling Vessels, which, in like manner with its Nerves, proceed from the *Cutis*, and terminate here, may be expanded into a Membrane at their Termination; of which Vessels there seem to be three more Kinds; viz. the *Vasa arteriosa* or *exhalantia*, the *venosa* or *inhalantia*, and the *Vasa excretoria*, or sudorific Ducts form the Glands of the Skin; each of which proceed from the *Plexus* of Vessels or Glands, of which the Skin is composed; and, perforating the *Corpus reticulare*, terminate in a thin membranous Expansion; which, from the several *Lamellæ*, or *Strata*, of scaly Membranes, of which these two accurate Anatomists, *Cowper* and *Rysch*, have observed the *Epidermis* to be composed: So that, as the *Plexus* of Nerves, which form the *Corpus reticulare*, is expanded over the *Plexus* of Veins and Arteries which compose the *Cutis*, to make them sensible, by their exquisite Sense, of any external Injuries; so the membranous Expansion of these Nerves, at

their final Termination in the outer *Lamella* of the *Epidermis*, is laid over the open Orifices of these Vessels, at their final Terminations, to keep off the Injuries they might be exposed to, by an immediate Contact with the open Air, either in obstructing their Orifices, exsiccating their Substance, or exhaling their liquid Contents too fast, if deprived of such a Cover. From hence it will appear, how far the *Epidermis* may be said to be vascular or not; and that it is very probable, that this outer Coverlet or external *Lamella* of it, is a solid nervous Expansion, enjoying no Sort of Vessels, any more than the ultimate *Fibrille* of the Nerves themselves can be supposed to do.

PROPOSITION IV.

The Colour of Negroes does not proceed from any black Humour, or fluid Parts contained in their Skins; for there is none such in any Part of their Bodies, more than in white People.

It has been the general received Opinion, since *Malpighi's* Time, that the Cause of the Colour of Negroes is a Juice or Fluid of a black Colour, which lies between the *Epidermis* and *Cutis*, in some aqueous Vessels, which serve to lubricate those Parts; which Opinion I would be willing to submit to, as others seem to have done, as far as the best Authority ought to go; but *Malpighi* seems to propose this, rather as a probable Opinion to be more thoroughly examined, than as an established one to be confided in; as would appear from his own Words, *Ex quo transeunter deduco haud incongruam forte Nigredinis*

Nigredinis Ethiopum Causam, &c. (a): And I must own I was formerly of that Opinion, thinking that the sulphureous oily Parts of their Fluids were more attenuated and exalted by the Heat of the Sun, and so rendered black, as we know happens to Oil when well boiled, and to our Tongues in ardent Fevers. But this Opinion needs only to be well and more thoroughly considered, to be confuted: For, if we consider the ill Qualities, and pernicious Effects to our Bodies, of any such exalted sulphureous Juices, no one will imagine that any Animal can live in Health, with any such Fluids in his Body; since all the Fluids of the Body constantly circulate, and communicate with each other; for such sulphureous Juices seem to be the Cause of black Tongues in acute Diseases, and of the Blackness of Gangrenes in some measure, which we know soon prove fatal, unless removed. Besides, these Juices must be secreted from the Blood, which seems to have no more Disposition to turn black, in black than white People: And, as these black Juices lie in the Skin, it is very probable, that they might often be exhaled, especially in Sweating; and might leave the Skin destitute of its black Colour, in some measure, at such times; which it never is, but appears rather blacker at such times, than any other. And as this Humour must be secreted from the Blood, and constantly exhaled and renewed again, it is very likely, that it would be often obstructed in its Secretion, or altered in Colour, in some morbid Cases, like the other Humours, as well as

(a) Epist. Anat. Edit. Londin. p. 26.

evaporated sometimes; which however we never see it to be.

But, however well received this Opinion may be, as being consistent with common Notions, yet it is very unphilosophical to assign any thing as a Cause, which does not appear to exist; and I believe that none will be able to shew the Existence of any such black Humour in the Skins of Negroes; for their Sweat, or the Water in their Blisters, which would probably contain some of this black Humour, was there any such thing, is as clear and white as in white People: Besides that the Existence of any Vessels in the Skin, which are supposed to contain this Humour, is not well agreed to by Anatomists. Mr. Cowper says, he never could find any such, altho' carefully sought for (a); and none else have ever been able to shew them: For the common Anatomists take the *Corpus reticulare* to be a mucous Substance, as they call it, which serves to lubricate the *Papillæ nervosæ*, and contains this black Humour; but, altho' it is of a soft pappy Substance in Whites, yet it cannot be called a fluid *Mucus*, any more than the Substance of the large Nerves, or of the Brain, from when it arises, and which are of a softer and more mucous Substance than it; besides that, in Negroes, it still more resembles a solid Membrane, being to be torn off like the *Epidermis*; and as it arises from the nervous *Involucra*, or Expansion of nervous Fibres, it probably contains no Fluids, or none but what are most pellucid and clear.

Moreover, whatever this supposed black Humour may be, or in whatever it may be contained, it must be opaque, and the Fibres or Vessels of the *Epidermis* must be transparent, to shew this Colour thro' them; as we have shewn, that the Skins of Negroes, but not of Whites, are opaque (PROP. I. II.): But it is very probable, that none of the subcutaneous Juices are opaque, they being the ultimate Secretions of the subtillest Fluids of our Bodies, which, instead of being opaque and black, must rather become pellucid and white; *Etenim Corpora omnium opacissima, si Partes ipsorum in summam usque tenuitatem comminuantur, evadunt continuo plane perfecteque pellucida* (a). And the Skins of Negroes, being more callous or cartilaginous, must contain rather subtiler, and more pellucid aqueous Juices, than those of white People.

Furthermore, if there was any such Thing as a black Humour in the Skins of Negroes, no doubt but it might be drawn out by some means or other; but, altho' I have macerated the Skins of Negroes, and particularly the *Epidermis*, in warm Water, which readily dissolves the Juices of the Body, yet I never could extract any black Juices from them, by any such Maceration, or even by a more powerful Expression; no more than Mr. *Littre* (as is related) could do, by more powerful Dissolvents. Nothing seems more likely to extract this supposed black Juice, than the Action of Fire, or *Cantharides*, upon the Vessels which contain it, which abrade and tear

(a) Newton Opt. lib. II. Part 3. PROP. IV.

the Vessels and Fibres of both the Cuticle and *Corpus reticulare* from the Skin, but leave them both as black as ever they seem to have been, altho' they would, no doubt, extract whatever Juices they might contain; as we see plainly they do, by the large Blisters, raised by such Applications, from the abraded Vessels spewing out their liquid Contents; in which Blisters there are no Tokens of any black Humours in Negroes more than in whites, as I have often found upon proper Trials; altho' if there was any such black Humour contained in their subcutaneous Vessels, there is no Doubt but it would appear, in some measure, in the Water of their Blisters, as well as the yellow Bile does, when diffused thro' the Blood, and upon the Skin, as I had lately some Opportunities of observing.

But it seems to be a total Overthrow of this Opinion, that the Blackness of the Skin in Negroes should proceed from the *Corpus reticulare*, and *Lamella externa Epidermidis*, as we have proved, *Prop. III.* For I believe that none, who maintain this Opinion, will or can, with any Pretence of Facts, or Shew of Reason, suppose, that these nervous, scaly, and dry Parts contain any such Juices, if they contain any at all, as it is most probable they do not, being the ultimate Expansions of nervous *Fibrille*, at their final Terminations; for no other Parts appear black, but these Two; whereas did this Blackness proceed from any coloured Juices, the other Parts or Membranes of the Skin and Cuticle, which seem more fit to receive such coloured Juices, would appear black likewise.

From what has been said, we may see how well their Opinions are grounded, who derive the Cause
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of the Colour of Negroes, from an Addition of the the Bile, or other atrabilious Humour, as they are pleased to talk.

PROPOSITION V.

The Epidermis, especially its external Lamella, is divided into two Parts, by its Pores and Scales, two hundred times less than the Particles of Bodies, on which their Colours depend.

Sir *Isaac Newton* informs us, that the Particles of Bodies, on which their Colours depend, are about 600 times less than those which can be discerned with the naked Eye; *Opt.* lib. II. *Part* 3. *PROP.* 7. But *Leeuwenhoeck* shews, that a Portion of the *Epidermis*, no bigger than what can be discerned with the naked Eye, is divided into 125,000 Pores; which Pores must divide such a Portion of the Skin as can be discerned with the naked Eye, into 125,000 Particles; therefore each of these Parts of the Skin, between its Pores, must be about two hundred times less than those Particles, on which the Colours of Bodies depend; for $\frac{125000}{600} = 208\frac{1}{3}$; not to mention, that such a Portion of the *Epidermis* is further divided into 250 Scales, which must increase the Number of Parts into which it is divided. Nor will any one think, that the Smallness of these Parts and Pores exceeds Credibility, who considers, that they convey the minutest Particles of our last Digestions; and were they even large enough to convey the Particles of many Waters, it is very probable, that all our Fluids might in time evaporate thro' them. Nor is it any thing to our present Purpose, whether these Numbers be mathematically

thematically exact or no; all that I would endeavour to prove, is, that the Parts into which the Cuticle is divided, are less, in some Proportion, than the Particles of Bodies on which their Colours depend.

PROPOSITION VI. *Problem.*

To determine and explain the proximate Cause of the Colour of Negroes, Indians, white People, &c. from the foregoing Propositions.

We have shewn above PROP. I. that the Colour of white People depends upon the Colour which the *Epidermis* transmits, and not on what it reflects: This Pellucidity of Bodies proceeds from the Number of Interstices between the Particles which compose them, and the extreme Smallness of those Particles; for, in order to render a Body of any Colour, or fit to reflect the Rays of Light, its Particles, and the Intervals between them, ought not to be less than a certain Magnitude (*a*); else they become incapable of making any Reflexions, from their common Surfaces, *i. e.* of appearing coloured: But, by PROP. V. the Cuticle is divided into Parts, and Pores or Intervals between these Particles, far less than those on which the Colours of Bodies depend; that is, too small to reflect any Rays of Light from their common Surfaces, or to appear coloured from such reflected Rays: But, as such porous Bodies are always transparent, so the *Epidermis* is transparent enough, to shew any Colour reflected from the Parts

below it: So that we must look upon the *Epidermis* of white People to be a transparent Pellicle, of too subtile or rare a Substance, and too minutely divided, to reflect any Rays of Light from its Surface; but consisting of Numbers of Pores, which readily transmit those Rays, through its thin and rare Substance, by which it shews the Colour of whatever Parts are below it; on which the Colour of white People depends.

But, as there are Numbers of Scales, or several *Strata* of scaly *Lamellæ* in the *Epidermis*, so this Transmission of the Rays of Light, from the subcutaneous Parts, must be imperfect, some of these Rays being intercepted in passing thro' the several *Lamellæ*; and the thicker the Cuticle is, *i. e.* the more there are of these *Lamellæ*, or the denser their Texture, the more the Light will be intercepted in passing them, and the more the Colour of the Skin will degenerate from the pure White of the Membranes below it. This is agreeable to Experience; for Mr. *Cowper* tells us, in his Anatomy, That the Thickness of the Skin proceeds from the Number of the *Strata* or Layers of Scales which compose it: And we may daily observe, that those who have such thick and coarse Skins, are never of so perfect and pure a White, as they who have a thin and fine Skin (as *(a)* *Cowper* observes). But the Reason why such thick-skinn'd People appear of a yellowish or tawny Colour, will be plain, from *Newton's* Observations (*Opt.* lib. II. p. 1. *Obs.* 9. and 20.) ; where he shews a faint yellowish

(a) Cowp. *Anat.* TAB. IV.

Colour to be the one that proceeds from an imperfect Transmiffion of a White; for no one can fay, but that both the internal Membranes and Humours of fuch fwarthy People are of the fame Colour in time of Health with thofe of the perfeft white Skins, as well as they are in Negroes. And this feems to be the Cause of the pale Yellow of dead Bodies, whofe Skins are not perfpirable, and, confequently not fo transparent, as in a living Subject.

From this Account of the Cause of the Difference in Colour among thofe People that are white, we may account for the Cause of the Colour of *Indians*, and other tawny People, who feem to differ from one another in Colour, and from white People only in Degree, as they have more or lefs of this tawny Yellow proceeding from the imperfect Transmiffion of a White in their Colours: Thus if we proceed from the fwarthieft white Perfon to the paleft *Egyptian*, from thence to the faireft *Muftee*, *Molatto*, *Moor*, &c. to the darkeft *Indian*, we may plainly fee, that they differ from one another only *fecundum majus vel minus*, according as they have more or lefs of the original White in their Colour: And as we have fhewn this tawny Colour in white People to proceed from the Thicknefs or Density of their Skins, obftructing the Transmiffion of the Rays of Light; fo it is very plain, that the fame tawny Colour, in thefe other tawny People, which feems to be of the fame Kind, but different in Degree, muft proceed from a like Cause, that is, the Thicknefs or Density of their Skins; and, accordingly, it will be found, that all fuch People have Skins of a Thicknefs or Density proportional to the Whitenefs or Darknefs of their Colours. The particular

particular Manner in which this Opacity, or imperfect Pellucidity of Bodies is brought about, Sir *Isaac Newton* explains to us (*Opt. ib. PROP. II.*); where he shews, that the Opacity of Bodies depends upon the Multitude of Reflexions that are made in their internal Parts; but it is very plain, that the thicker the Skin is, the more Reflexions the Rays of Light must suffer in passing thro' it, by which they will be extinguished, in proportion to the Number of such Reflexions; that is, the more opaque, or less white, it must appear: So that, altho' the Particles, of which white and dark skinn'd People are composed, may not be very different from one another, as they seem not to be; yet a greater Number of such combined Particles, or more *Strata* of them, in thick Skins, and the Smallness of their Intervals in Skins of a dense Texture, will increase the Number of Reflexions made in their internal Parts, or the Opacity of them; which renders them less white, since their Whiteness proceeds from the Number of the transmitted Rays.

In the same Manner, by which we have accounted for the Colours of tawny People, may we account for the Colour of those that are black: For, if the Skin appears darker and darker colour'd, the more the Rays of Light are intercepted by it, of course it must follow, that when the Rays of Light are intirely intercepted by a Body of the same Structure, (which the Skins of Negroes seem to be) it must be quite black; for Blackness always proceeds from a Suffocation of the Rays of Light, as those versed in the Doctrine of Light and Colours are well acquainted with: But we have shewn above (*PROP. II.*), that
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the Skins of Negroes transmit no Colour or Rays of Light thro' them, on account of the Thickness of their Substance, and Density of their Texture, in the same manner as they are imperfectly transmitted in some white or tawny People, whose Skins appear to be of the same Structure with those of Negroes, and to differ from them in nothing, but in Degree of Thickness and Density, and in Colour; which different Density may therefore probably be one, if not the only Cause of this Difference of Colour. So that the Thickness and Density of the Skins of Negroes seems to be the grand Cause of their Colour, in the same manner as it is of *Indians*, *Moors*, &c. Which may be further confirmed by the following Considerations: 1. In the Cicatrices of their Ulcers, the thin and tender new-form'd Skin appears whitish, nay, perfectly white, in some, especially on the Shins, or those Places where these Cicatrices are thin; but, where the Skin is thick, or when these Cicatrices turn more thick and callous, they grow blacker in Proportion; as in those Places where the Scars grow thicker than the rest of the Skin, they are likewise blacker. 2. The Colour of the Water, contained in the Blisters of white People, may be plainly seen thro' their Cuticles, especially if tinged yellow, which cannot be perceived in the Blisters of Negroes: A plain Proof, that their Cuticles are not transparent, as those of white People are. 3. Infant-Negroes, whose Skins differ from Adults only in the Thinness and Rarity of their Texture, look whitish, in comparison to adult Negroes; but grow black, as their Skins turn thicker and denser. These Infant Negroes, labouring under an *Icterus*, look of a yellow Colour, all

all over their Body; which the Adult do not, except in the Eyes, as I had lately an Opportunity of observing: A plain Proof, that the Colour of the Skin proceeds from the Colour which is transmitted thro' it; and that the Skins of adult Negroes transmit no Colour of any Sort. 4. But that the Thickness of the *Corpus reticulare*, the Part which appears black in Negroes, by PROP. III. may and does make it black, *Malpighi* give us an Instance in the said Part, in the Tongue of a Beef, in which it appears black, on the middle of the Tongue, where it is thick; but is white on the Edges and Checks, where it is very thin (a). As for the Manner in which this Blackness or Opacity is occasion'd by a thick or dense Skin, it will appear from what has been said about the Skins of tawny People: And it is very easy to conceive how the Rays of Light are intercepted, in passing thro' the thick and dense Skins of Negroes, which easily pervade the thin and rare Cuticles of Whites.

But, as the Skins of Negroes are of a denser Texture than those of Whites, they will be more apt to refract the Rays of Light; for the denser the Body, the greater the Power of Refracting (b); and the greater the Refraction of any Body is, the more apt it will be to absorb the Rays of Light; which is another Property of opaque Bodies, by which they become black: *Porro quo corpora videantur nigra,*

(a) *Malpighi de Lingua*, p. 15, 16.
PROP. X.

(b) *Newton Opt.*

neceſſe & multi Radii intercipientur, reſtinguantur, & inter ipſa intercitant (a).

Two Properties are generally aſſigned to all black Bodies, to be opaque and porous: Now the Skin is well known to be porous; and we have ſhewn, that the Skins of Negroes are opaque; to theſe we may add a third neceſſary Property of black Bodies, *viz.* the Minuteness of their Particles: For, as Sir *Iſaac Newton* ſhews, *Ad Nigrorem exhibendum, Particulæ adhuc minores eſſe debent omnibus illis, quæ Colores cujuſcunque modi exhibent: Nam Particulæ omnes majusculæ plus refleſtunt Luminis, quam ut nigræ poſſint videri (b)*; which Smallneſs of Particles we have above demonſtrated in the Skin (PROP. V.); and it is probable, that, in Negroes, the Particles of the Skin between the Pores are ſmaller than in Whites; as the Pores themſelves are ſo: From which Smallneſs of its Particles, the Skins of Negroes cannot reflect the Rays of Light; -- another Cauſe of their being black.

So that, from the Whole, we may conclude, that the proximate Cauſe of the Colour of Negroes is threefold; *viz.* the Opacity of their Skins, proceeding from the Thickneſs and Density of their Texture, which obſtructs the Tranſmiſſion of the Rays of Light, from the white and red Parts below them; together with their greater refractive Power, which abſorbs thoſe Rays; and the Smallneſs of the Particles of their Skins, which hinder them to reflect any Light. Q. E. I.

(a) *Newton Opt. PROP. V. II.*

(b) *Ibid. PROP. VII.*
SCHOLIUM.

SCHOLIUM.

I doubt not, but that those who are used to account for the Colours of all consistent Bodies, from the Reflexion of those Colours from their Surfaces, which is the common Way of accounting for the Colours of solid Substances, without noticing the Colours of pellucid Bodies; and are used to derive the different Colours of the Skin from differently coloured Fluids, appearing thro' its pellucid Vessels, as in most morbid Cases; may not be intirely satisfied, at first, with this Account of the different Colours of human Bodies being occasioned, as I have affirmed, by the more or less perfect Transmission of the Rays of Light thro' their Cuticles; which, from the different Thickness and Density it is observed to be of, is more or less fit to transmit those Rays; the more of which being intercepted, the darker the Skin appears; till, at last, being intirely intercepted by the thickest and densest Skins, as those of Negroes are, it appears, of no Colour, or looks black. But let such consider, that if the Whiteness of the Skin, in Whites, proceeds from the Thinness of its Substance, Rareness of its Texture, the Numbers of its Pores, and Minuteness of its Particles, by which Structure it transmits the Rays of Light; as is very probable, from this and the I. Proposition: If so, I say, there will be no Reason to suppose, but that the Colours of *Molatto's*, *Indians*, and Negroes, proceed from a similar or like Cause, and not from any particular new-induced Texture, by which their Skins may become less able, or intirely incapable, to reflect the Rays of Light; since their Colours seem

to differ from one another only in the Degree of one and the same Colour; and the Structure of the Skin seems to be intirely the same in both, except in the different Thickness or Density of it; which seems more fit to induce a Change of Colour from the different Transmission of the Rays of Light, than from the different Reflexions of it: For the ultimate component *Strata* of Scales, of which the Cuticles of both white and black People are composed, seem to be intirely the same in both, or, at least, but little, if at all, altered in their reflecting Powers, whatever they may be in their Opacity, or Pellucidity, from the different Assemblage and Combinations of them: Add to this, that the Colours of even the fairest Skins look more like a Colour proceeding from transmitted, than reflected Light; being more languid, or not so brilliant, as those Colours that proceed from such reflected Rays: For this Reflexion, even from the *Epidermis* itself, is of a shining silver White, as we have above remarked. Moreover, the denser and thicker the Body is, whose Colour proceeds from reflected Light, the more vivid its Colour will be, such Bodies being most capable of reflecting the greatest Quantity of the Rays of Light; but we have demonstrated above, that the thinner and rarer the Skin is, the more clear and bright its Colour is; which does not therefore, in all Probability, proceed from such reflected Light. I know very well, that the different Smoothness or Roughness of the Surface of Bodies tends much to brighten or obscure their Colours; but the darkest Skins, as well as those of Negroes, seem to be as sleek and smooth on their outer Surfaces, as those of the fairest Colour.

But,

But, as this Cause of the Diversity of Colours, which we have assigned, seems to be the most simple, and easily effected, like the Operations of Nature constantly observable in other Things, so it is most consistent with the Doctrine of Colours in many other respects; for, were not our Colours to proceed from the Parts under the *Epidermis*, which are always of the same Colour in all People of the most different Complexions; but the Skin was to be a solid opaque Body, which reflected our Colours from its Surface, like most other coloured Bodies; we should then, in all Probability, have People of all the different Colours in the Rainbow, and that in one and the same Nation; for the Skin is seldom observed to be of the same Thickness and Density in different Persons of the same Nation: But Sir *Isaac Newton* shews us, that those Bodies, whose Colours proceed from reflected Light, when they alter their Thickness and Density, change their Colours, not to a more or less perfect one of the same Kind, but to one of a different Kind (*a*): Thus a thin Plate of Talc, whose Colour proceeds from the Light reflected from its Surface, alters its Colour, according to its Thickness, to all the different primary Colours; and one of these Plates, of a pale yellow Colour, laid over another one of a blue Colour, turns of a deep Purple (*b*): This would probably be the Case of our Skins, was its Colour to proceed from reflected Light; since there are many different *Strata* of scaly Plates laid over one another in the Skins of different Persons, even

(a) Newton. *Opt.* p. 195, &c.

(b) *Idem ibid.* p. 196.

of the same Nation, as Mr. *Cowper* informs us (a): And every one may perceive, that the Skins of different Persons are of different Thickneses and Densities; and much more those of different Nations and Complexions: But when the Diversity of our Colours proceed from the different Transmissions of the Rays of Light, from one and the same coloured Bodies, such different Thickneses of our Skins will only make our Colours vary from one another *secundum majus vel minus*, only in the Degree of one and the same Colour; by which alone will Negroes, *Indians*, and white People, or each of a Sort, differ from one another in Colour; and, consequently, their different Colours will proceed from Causes more uniform and alike, agreeable to the exact Symmetry of Nature in the Variations of other Things of the same Kind; so that, however different, and opposite to one another, these two Colours of Black and White may appear to be to the Unskilful, yet they will be found to differ from one another only in Degree; since Whiteness proceeds from a Reflexion or Transmission of the Rays of all Colours; but Blackness is brought on, by an Extinction or Suffocation of those same mixed Rays, which, probably, in the black Bodies, are reflected or transmitted in some small Quantity, as they are in larger or smaller Quantities, in proportion to the Whiteness of Bodies (b): And hence it is, that one of these Colours is more easily changed into the other, than to any other Colour; and where any Body loses its white Colour, it of course turns black, without any other Cause concurring, but a bare Loss of its Whiteness (c). From

(a) *Anatom.* Tab. IV. (b) *Newton Opt.* ubique. (c) *Idem ib.*
whence

whence we may justly infer, 1. That there is not so great, unnatural, and unaccountable a Difference between Negroes and white People, on account of their Colours, as to make it impossible for both ever to have been descended from the same Stock, as some People, unskilled in the Doctrine of Light and Colours, are very apt too positively to affirm, and, without any Scruple, to believe, contrary to the Doctrine (as it seems to be) of the Sacred Pages. 2. That the *Epidermis*, besides its other Uses, tends to preserve the Uniformity of the Colours of People throughout the World.

PROPOSITION VII.

The Influence of the Sun, in hot Countries, and the Ways of Life of the Inhabitants in them, are the remote Causes of the Colour of Negroes, Indians, &c. And the Ways of Living, in Use among most Nations of white People, make their Colours whiter, than they were originally, or would be naturally.

My Design in this Essay was, not to treat fully of every thing relating to the Causes or Effects of the Colours of People, but only to inquire into the particular Make and Structure of the Skin, by which it comes to appear of so many and so diverse Colours in the different People of the World; and to explain, from *Newton's* Doctrine of Light and Colours, in what Manner these different Colours are produced from this particular Make and Structure of the Skin; which is, what I take to be, the grand Question relating to the unknown Cause of the Colour of Negroes; which has been often attempted to be determined,

mined, but with little Satisfaction to the Learned : For which Reason I have been more particular and explicit in delivering any Thoughts about it, as I have never known or heard of any such to have been delivered by any others. As for what relates to the remote Causes of the Colours of Negroes, it has been generally supposed ; although not universally believed, that the Power of the Sun in hot Countries is the principal, if not the only, Agent in producing this Effect : But, as the Authors of this Opinion seem not to have understood what Effect or Alteration is produced in the Make of the Skin, in order to render it black, so they have not been able to satisfy any one in this Point, and far less to vindicate their Opinion from many material Objections ; for it would be very difficult, if not impossible, to shew or prove, in what manner the sole Effect of Blackness is occasioned by the Power of the Sun, but not so difficult to shew how it may make the Skin thicker or denser ; which we shall endeavour in the next place to do, by shewing in what Manner the Power of the Sun is able to cause that Thickness and Density of Skin, which we have assigned as the immediate Cause of its black or tawny Colour. But as this Subject is much plainer, and more obvious, than the other, concerning the Make of the Skin, or the immediate Cause by which its Blackness is occasioned, so I shall not dwell long upon it, but only give the principal Heads of these Arguments, which serve to prove this Proposition : Which Arguments are of two Sorts ; viz. philosophical and historical ; the first of which I shall chiefly touch upon, being what are least understood, or, at least, hardly to be met with any where else. The

The Proof of the first Part of this Proposition will consist, chiefly, in shewing what Effect of the Sun it is which deprives the Skin of its white Colour, rather than what it is which causes it be black; for, to prove the Cause of Blackness, is the same as to prove a Negative; Blackness being a Negative with regard to Colour. The Skin then is deprived of its white Colour, by the Force and Influence of the Sun, these four Ways:

1. By being rendered opaque, from a Dissipation of its more aqueous and pellucid Juices. The known Effect of the Sun's Heat, and which will render all Bodies opaque: *Nam corpora ea, quæ sunt maxime pellucida, poterunt, occultorum suorum meatuum evacuatione, satis opaca evadere (a).*

2. By a Concretion of its Vessels and Glandules, from this Dissipation of their aqueous Contents, which renders the Skin both thicker and denser, or more callous or rigid. For the Skin being designed as a Defence to the other subcutaneous Parts, as the *Epidermis* is to the *Cutis*, they both wonderfully accommodate themselves to the Nature and Force of external Injuries, so as to become capable to defend the Body from them; as we see in Smiths, &c. constantly used to handle hot and hard Things, who have the Skin of their Hands become so thick and hard or cartilaginous by it, as to be able in time to handle even hot Irons: And thus it is, in a great measure, with the Skins of Negroes, *Indians*, &c. constantly exposed, and generally naked, to the scorching Heat of the Sun in a perpetual Summer.

(a) Newton Opt. 16. PROP. III.

3. By a new Accretion of many new Membranes, which render it thick and opaque. For the Sunbeams act as a vibrating Force, or external Friction, upon the Skin, which derives fresh Supplies of Juices to it; by which new Membranes, or *Lamellæ*, are formed, in the same manner as the *Epidermis* is renewed when abraded, which is very soon and easily done. This is the Sense of a very great Philosopher; *Calor solis homines quibusdam in regionibus nigricante colore tingit, ut in Æthiopia, Guinea, &c. Non esse illud ignis effectum ostendunt vitrarum, continuo ad ignem occupati. Ratio, fortasse, quod ignes in sanguinem & spiritus agat ut exhalent, hominesque sic reddet pallidos & subfuscios: benignior autem solis calor sanguinem duntaxat in corporis extimas partes proleat, concoquitque eum potius quam eliquat (a).* Which Derivation and Concoction of the Humours on the Surface of the Body must occasion a Thickness of their Skins, as well as of their Lips, and other Muscles, especially of their Face.

4. By increasing those Parts or Principles, in the Composition of the *Epidermis*, which have the greatest refractive Powers, As the terrestrial, and fixed saline; but, especially, the tenacious sulphureous, which refract and absorb Light more strongly than any other Substances (b); whilst the more transparent and pellucid Principles, as the aqueous, spirituous, and volatile saline, are evaporated by the Heat, which causes the other more fixed Principles to be accumulated in

(a) Bacon *Hist. Natur. Cent. IV.* 399.
PROP. X.

(b) Newton *Opt.*

greater Quantities, and combined in larger Collections; and these Particles, being likewise more comminuted by the Sun, will on that account be black, as happens to Oil when well boiled.

From what we have said above about the immediate Causes of the Colours of the Skin, it will appear, that these several Effects of the Sun's Heat contribute to make it of a darker Colour; and no one will doubt, I believe, but that all of them, conspiring together, may make it quite black.

To these, perhaps, might be added another Effect of the Sun's Power, a peculiar *Necrosis* of the *Epi-dermis*, occasioned by the forcible Vibrations, Contractions, and Exsiccations of its Fibres by the Sunbeams, which cause it to turn black, as these, or the other Parts, do by the Heat of an Inflammation or a Fever, in Gangrenes, black Tongues, &c. From whence only the nervous Parts of the Skin come to be black, and more hard and callous, and less pellucid, than the rest; and the Skins of Negroes, besides their Callosity, become more insensible than those of Whites.

But as there are many Degrees of Whiteness and Blackness in the Colours of the People in the World, depending upon the different Densities and Thicknesses of their respective Cuticles, as we have above shewed, it may not be improper, in the next place, to inquire into the more peculiar Causes of this Diversity, which will be found to be such as increase or diminish the Power of the Sun's Heat, or its Influence on the Body; by which the only material Objection that has been brought against this Proposition may be answered; *viz.* that the Sun's is not

the Cause of Negroes, because several Nations of People, in the same Latitude with the Negroes in *Africa*, are not made black by it.

The Causes of this Diversity may be referred to two Heads; *viz.* 1. The Nature and Temper of the Country. 2. The Ways of Living in it. Under the first may be included the following Particulars:

1. The Nature of the Soil, and Situation of the Country, with regard to Mountains, Waters, &c. which very much alter the Power of the Sun's Heat; for the differing Degrees of Heat and Cold, in different Places, depend, in a great measure, upon the Accidents of the Neighbourhood of high Mountains, whose Height exceedingly chills the Air brought by the Winds over them; and of the Nature of the Soil, which variously retains the Heat, particularly the sandy; which, in *Africa*, *Arabia*, and generally where such sandy Deserts are found, do make the Heat of the Summers incredible to those who have not felt them; as the learned Dr. *Halley* has remarked. Whence it will appear, that the Heat or Influence of the Sun is not always the same in the same Latitudes, as they imagine who start this Objection to this Proposition; but that in *Africa*, where the People are black, the Soil is as intemperately hot as the Climate, occasioned by the scorching Heat of its Sands, according to the just Account of *Lucan*,

— *per calidas Libyæ sitientis arenas*;

agreeable to the Accounts of all Travellers and Historians, especially those who talk of its inland Parts, where People first began to turn black. This Heat of the Soil must much increase the Heat of the Sun, and its Power upon the Body: And if the Sun is the Cause of

Black-

Blackness, must make the People blacker in such Places than any-where else ; which we see to be true of the Negroes in *Africa*, who are much blacker than the *Indians* of *Asia*, or *America*, who live in the same Climate, but inhabit more temperate Countries. This Power of the Sun will be much increased in such sandy Soils,

2. By the Scarcity, if not intire Absence, of large, spreading, succulent Plants; which afford, in other moist and more fertile Soils in hot Countries, agreeable cooling Shades, or a moist cool Atmosphere, from their Exhalations, which take off much of the scorching Heat of the Sun; which *Lucan* observes to be the Case in *Africa*:

Hoc tam segne solum raras tamen exerit Herbas.

3. The Want of Water must much increase the Heat of the Body, if not of the Sun; and conspires to the same Effects, as the more immediate Heat of the Sun itself. This is well known to be the Case in *Africa*, from the many Caravans that perish for Want of Water in travelling through its midland Parts (*vide* Geog. Nat. *Leo Hist. Africæ*; as well as the Account we have of *Cato's* March thro' it, and many other Travellers): Besides, it rains so seldom in many Places of *Africa*, as to make it generally believed, formerly, that it never rained there at all; which must much more exsiccate the Body, and parch the Skin more powerfully, in these sandy Regions, where no Rain ever falls, but at a certain Season or two in the Year, than in more temperate Regions, altho' in the same Latitude.

2. The Ways of Living in many hot Countries, particularly in *Africa*, do very much contribute to

increase the Influence of the Sun upon the Body, or to thicken and harden the Skin, upon which its Blackness depends. These Customs are,

1. The Custom of going naked among most of the Nations of *Africa*, especially those that are black, both in former Days, as well as at present.

2. Living not only without Cloaths, but also without Houses, in a very barbarous and rude manner, little better than the wild Beasts; as the *Casars* do at this Day all over *Africa*; and was the Custom of the *Nomades*, *Troglodytæ*, *Numidians*, and many other barbarous Nations of old (a).

3. The Custom of wandering up and down in these sandy Deserts, in the scorching Heat of the Sun, stark-naked, with no House or cool Shade to retire to, nor Water to refresh themselves with, or cool their Bodies in:

*Nulla domus; plaustri habitant; migrare per arva
Mos, & errantes circumvectare penates.*

4. The Custom of most People in hot Countries of Anointing their Bodies with some greasy and unctuous Epithems, to defend their Skins from the scorching Heat of the Sun, will be found likewise to increase the Darkeness of their Colours.

On the contrary, the Customs and Ways of Life in Use among the *Europeans*, and other Nations of fair Complexions, contribute to render their Skins whiter than they otherwise would be, or than they were, in all Probability, originally. These Customs seem to be, an almost constant Confinement, or rather Imprisonment, from the open Air; warm and soft
Cloaths,

(a) Lud. Hist. Æthiop. lib. 1. cap. 14. Plin. Hist. &c.

Cloaths; warm Beds; Sitting by Fires; the Custom of Bathing much in Use formerly; a more succulent and nourishing Diet; Excess in strong Liquors; frequent Sipping of warm thin Liquors; and, in general, more luxurious and effeminate Lives; all which, with the Absence of the Sun, or Defences from it, tend to soften, moisten, and relax, the Fibres of the Body, and to render the Fluids more thin and watery; and, consequently, the Membranes composed of them, such as the Skin is, must be more clear and transparent; on which, we have shewn, its Whiteness depends; and, accordingly, we constantly see, that People of such Constitutions, or Ways of Life among us, are always the whitest. We might, indeed, consider the Effects of Cold upon the Skin in these northern Climes, where the People are white, were it not that those, who are the fairest among them, are the least exposed to it, and seldom or never feel its Effects; but the Whiteness of their Complexions seems rather to be occasioned by muffling themselves up against the Cold, than from being exposed to its Influence: For, as the *Cuticula* is a Sort of Cloathing to the other Membranes of the Body, and, by preserving the Whiteness of them, serves, besides its numerous other Uses, to keep up an Uniformity and Harmony in the Colours of People; so there is no Doubt, but, that the Cloaths wherewith we cover it, preserve its Whiteness, or render it whiter, as every Fair-one knows: So that the different Customs of different Nations, in this respect, will tend very much, besides other Causes, to make that Alteration and Diversity so observable in their Complexions. So that it seems to be but a small Objection, if any at all, to this Proposition,

That the Natives of *Canada*, altho' but a cold and northern Clime, are of a swarthy Colour, whilst others, in the same Latitude in *Europe*, are white ; for the Customs and Ways of Life of these last seem very much to increase, if not occasion, the Whiteness of their Colours ; whereas the hard Lives, and savage Customs, of these *Canada Indians*, especially their going stark-naked all over (a), seem to have no Tendency to soften their Skins, or refine their Complexions ; not to mention their Custom of intermixing with the captive Women of southern Nations. But as the *Canada Indians* are the most northern, so they are the palest, of all *Indians*.

Hence it will appear, that the Power of the Sun's Heat in hot Countries, and its more immediate Application to the Body, or the Increase of its Force, by the Nature of the Soil, or Ways of Life, is the remote Cause of the Blackness, and the different Degrees of Blackness, of the Inhabitants of the Torrid Zone : Whereas the luxurious Customs, and the effeminate Lives, of the several Nations of white People, in the northern Climes, are the remote Causes of their respective fair Complexions. *Q. E. D.*

Altho' I have gone much further in this Epistle, than I expected or intended, or my Time would well permit, or the Nature of an Epistle would well bear ; for which Reason I have passed over what others may have imagined to be the Cause of the Colour of Negroes, and have abridged every thing as much as I well could ; yet I cannot but take notice, that as the Knowledge of any Cause is always conducive to investigate,

(a) La Hontan. *Voyage*, vol I. lett. 16. vol. II. cap. 1.

investigate, and account for, many Effects depending on, or proceeding from, these Causes; so this Account of the Structure of the Skins, and Colour of Negroes, and other tawny People, if duly and attentively considered, will lead us to the Knowledge of many intricate *Phænomena*, in Nature and Diseases, either unknown, or not so easily accountable for before; whose Consequences, as they are of the greatest Concern to us, so I am unwilling to pass them over intirely in this Place; altho' a particular Discussion of each would require so much more Room, as to make the Tail of this Discourse bigger than the Body. I shall, therefore, only give you the principal Heads of what may be rationally deduced from the foregoing Propositions, as so many Corollaries from them, reserving the particular Discussion of each to another Opportunity; which as I have already framed to myself, from many convincing Observations, which I have made here in *Virginia*, so I shall present you with them, if you find these my Reasonings and Observations either agreeable to the Rules of sound Philosophy, or of public Utility.

Coroll. I. White Spots on the Skins of Negroes are as common, and proceed from the same Causes with red Spots on white People; *viz.* a Distention; Dilatation, and consequent Rarity or Pellucidity, of the *Vascula* of the *Epidermis*: From whence the Physical Causes of the total Whiteness of some Negroes, at their Birth, may be accounted for (a).

Coroll.

(a) *Vide Hist. Cariclee apud Heliodor. of which I have seen an Instance in Virginia.*

Coroll. II. The Hair of Negroes becomes short, stiff, and frizzled, from the Exsiccation of its Substance, and its excrementitious Moisture, by the Heat of the Sun; together with the Thickness and Density of the *Pericranium*, which hinders it to be further protruded.

Coroll. III. Many morbid Discolorations of the Body proceed rather from a preternatural Thickness and Density of the Membranes of the Skin, than from any Humours lodged in them, as is commonly supposed; and may be accounted for in the same manner, as the different Complexions in Time of Health.

Coroll. IV. The Bodies of Whites are more perspirable, than those of Negroes, but perspire less in hot Weather, and more in cold.

Coroll. V. White People are most healthy in cold, and black or tawny People in hot Countries; each being subject to Disorders, on a Removal to these respective Climes. ---- The Causes of the Diseases of white People in hot Countries are often opposite and contrary to such as proceed merely from Heat, which exalts the Fluids, exsiccates the Solids; and quickens the Circulation, occasioning severe acute Diseases; but the thin and rare Skins, and large Pores of white People, make them subject to too large cutaneous Evacuations of the most subtil and active Fluids; by which the Body is imbecilled, and comes to be in an imbibing State, both on its external and internal Surfaces; and too readily imbibes the Humidity of the Air and Aliment, without a previous Digestion; causing a cold and humid, rather than a hot and dry, State of the Body;

Body; from whence proceed their lingering acute, and obstinately chronical, Maladies, more frequent in hot Countries than the former, especially among the Whites. ----- Negroes, notwithstanding their hardier Usage, are more apt to have their Perspiration obstructed in cold Weather, and contract Fevers from thence; whereas, in hot Weather, their thicker Hides serve as a Coat, to keep off the Power of the Sun, and preserve the Body against the Moisture of the Air, so remarkably great, and very pernicious, in all hot Countries, especially at certain Seasons, which are always sickly.----Hence, white People should be best cloathed in hot Weather, and Blacks in cold; a Thing much neglected in *Virginia*, tho' the Cause of one half of the untimely Deaths of both Sorts of People in it.

Coroll. VI. The perspirable Matter of black or tawny People is more subtil and volatile in its Nature; and more acrid, penetrating, and offensive, in its Effects; and more of the Nature, and more apt to degenerate to a contagious *Miasma*, than the milder *Effluvia* of Whites.---The Contagion of pestilential Fevers proceeds from a Subtilization and Volatilization of the perspirable Humours, by the Effects of a preceding Fever, as often, if not more often, than from any external Putrefaction, or mineral Exhalation.---Hence this Acrimony of the perspirable Humour of black and tawny People makes them subject to malignant and pestilential Fevers, from the same Causes which breed only putrid benign Fevers among Whites; and in them these Fevers are more apt to turn contagious, as they themselves are to be infected with such Contagion. --- From hence seem to have proceeded the first Seeds of the

Measles and Small-Pox, with the *African* or true Plague. --- From hence likewise proceeds the rank Smell, or peculiar *Fætor*, of dark-skin'd People.

Coroll. VII. This Acrimony of the perspirable Humours, with the Thickness and Density of the Skins of black and tawny People, or Imperspirability of their Bodies, makes them subject to many severe cutaneous Diseases, accompanied with a Contagion, which white People never feel, but by Infection from them; and then these Diseases appear in other Shapes, with milder Symptoms, than in the dark-skin'd People which breed them. --- These Diseases, which I have observed, among them, may be referred to the *Elephantiasis Græcorum*, or *Lepra Arabum*, two Species of which are called, the *Taws*, and the *Joint-Evil*; with some others, not named, appearing in obstinate subcutaneous Ulcers. --- But the *Elephantiasis Arabum*, to which the Negroes are likewise subject, is not a cutaneous Distemper, as has been thought, but a peculiar Kind of Cachexy, accompanied with an atrabilious Cacochymy, as in those afflicted with the Hemorrhoids; that being much the same Distemper in the Legs, as this is in the Hemorrhoidal Veins. --- The peculiar Diseases of white People analogous to these of the Blacks, and which the Blacks never have, are the *Lepa Græcorum*, at least with furious Desquammations, the Itch, Scurvy, Efficere, and some smaller ones of that Kind. --- This cutaneous Malady of the Negroes, called the *Taws*, laid the first Foundation of the *Lues Venerea*; which became to differ from it only by the Part affected,

affected, and the particular Manner of receiving the Infection, after being transplanted into another colder Clime, on People of a different Complexion; the virulent Acrimony of the cutaneous Contagion being inviscated, and consequently mitigated, by the *Semen* which received it; the subtiler Parts of the Contagion being likewise exhaled in the white People, on account of the Perspirability of their Bodies, although the Distemper was drove more on the internal Organs, upon account of the Coldness of the Climate; and so appeared to partake less of a true cutaneous Malady, after this *Lues Venerea* was first propagated to *Europe*. Hence it is, originally, a cutaneous Malady, only to be cured as such; the Venom which attends it, and gives Rise to it, being to be evacuated, most surely and effectually, by the Pores of the Skin, as it was originally bred by the acrid *Effluvia* which pass thro' them. -- Hence the Nature, Origin, Progress, Alterations, and different Success of divers Methods of Cure of this *Lues*, may be accounted for; and the most rational Methods of Cure deduced.

Coroll. VIII. From what has been said about the Cause of the Colour of black and white People, we may justly conclude, that they might very naturally be both descended from one and the same Parents, as we are otherwise better assured from Scripture, that they are (a); which may remove the Scruples of some nice Philosophers on this Matter, who cannot or will not believe even the Scriptures,

(a) Genes. III. 20. IX. 19.

Scriptures, unless it be so far as they can be made agreeable to their Philosophy: For the different Colours of People have been demonstrated to be only the necessary Effects, and natural Consequences, of their respective Climes, and Ways of Life; as we may further learn from Experience, that they are the most suitable for the Preservation of Health, and the Ease and Convenience of Mankind in these Climes, and Ways of Living: So that the black Colour of the Negroes of *Africa*, instead of being a Curse denounced on them, on account of their Forefather *Ham*, as some have idly imagined, is rather a Blessing, rendering their Lives, in that intemperate Region, more tolerable, and less painful: Whereas, on the other hand, the white People, who look on themselves as the primitive Race of Men, from a certain Superiority of Worth, either supposed or assumed, seem to have the least Pretensions to it of any, either from History or Philosophy; for they seem to have degenerated more from the primitive and original Complexion of Mankind, in *Noah* and his Sons, than even the *Indians* and Negroes; and that to the worst Extreme, the most delicate, tender, and sickly. --- For there is no Doubt, but that *Noah* and his Sons were of a Complexion suitable to the Climate where they resided, as well as all the rest of Mankind; which is the Colour of the southern *Tartars* of *Asia*, or northern *Chinese*, at this Day perhaps, which is a dark swarthy, a Medium betwixt Black and White: From which primitive Colour the *Europeans* degenerated as much on

one hand; as the *Africans* did on the other; the *Asiatics* (unless, perhaps, where mixed with the whiter *Europeans*) with most of the *Americans*, retaining the primitive and original Complexion. --- The grand Obstacle to the Belief of this Relation between white and black People is, that, on comparing them together, their Colours seem to be so opposite and contrary, that it seems impossible that one should ever have been descended from the other. But, besides the Falsity of this supposed direct Contrariety of their Colours, they being only different, altho' extreme, Degrees of the same Sort of Colour, as we have above proved; besides this, I say, that is not a right State of the Question; we do not affirm, that either Blacks or Whites were originally descended from one another, but that both were descended from People of an intermediate tawny Colour; whose Posterity became more and more tawny, *i. e.* black, in the southern Regions, and less so, or white, in the northern Climes: Whilst those who remained in the middle Regions, where the first Men resided, continued of their primitive tawny Complexions; which we see confirmed by Matter of Fact, in all the different People in the World. --- Agreeable to this, we see that the Heat of the Sun will tan, as the Saying is, the fairest Skin, of a dark swarthy, even at this Day; in which there is some Degree of Blackness; or, at least, this may well be said to be a Tendency to their primitive swarthy Complexions; *Rubescere cum nigredine quadam inceptit*, says

say *Sennertus* (a). So that if the Heat of the Sun will turn a white Skin swarthy, as nobody in hot Countries can doubt, the same Cause might turn the swarthy and tawny black; for the Effect seems to be the same in one as in the other, and may therefore be produced by one and the same Cause.--- As for the black People recovering, in the same manner, their primitive swarthy Colours of their Forefathers, by removing from their intemperate scorching Regions, it must be observed, that there is a great Difference in the different Ways of changing Colours to one another: Thus Dyers can very easily dye any white Cloth black, but cannot so easily discharge that Black, and bring it to its first Colour: And thus, altho' the Skins of white, or even swarthy People, are easily affected by the greater Power of the Sun's Beams than what they have been used to, and thereby become black; yet they are thereby rendered so thick and hard, or rough and callous, as not to be so easily affected, or readily wrought upon, to render them again of their original swarthy or pale Colour, by any of those Causes, as the Absence of the Sun, Coldness of the Climate, or Ways of Life in it, which we have supposed to be the Causes of the fair Complexions of the *Europeans*; altho', I believe, it has never been tried, what Effect these luxurious Customs, or soft and effeminate Lives, which we have supposed to be the Causes of Mankind's turning to so tender and delicate Complexions as the *Europe-ans*

(a) *Prax. Med.* lib. v. part 3. cap. 1.

ans have, and to be the Cause of all Whiteness in the Complexions of Men, or Changes from a dark to a fairer Complexion, might have upon the Colour of Negroes; but this we are assured of, that they are not of so deep a Black, in cold northern, as in the hotter southerly Regions. --- Besides, we want not some convincing Instances, from the Gleanings of the few Historians I was furnished with here, to shew that such Changes have happened in the Memory of Men, and within the Compass of those Records we have of Time; for we could not suppose it to have happened all at once: Thus *Herodotus* tells us (a), That the *Colchi* were formerly black, with frizzled Hair; which (he says) he relates rather as a Thing well known before, than a bare Report; but there is no Sign of any Blackness in the Complexions of their Descendants, they being rather, especially about *Circassia*, reckoned some of the fairest People in the World at this Day. --- Captain *Smith* tells us, that, even in *Virginia*, an *Englishman*, by living only three Years among the *Indians*, became "so like an *Indian*, in Habit and "Complexion, that he knew him not but by his "Tongue (b):" And what might his Children have turned to in a Succession of many Generations, by these same Ways of Life, which had so altered him in three Years? --- The *Moors* and *Lybians*, being drove out of *Africa*, upon the *Turkish* Conquest, retired to the Land of the *Negroes* (c);
but

(a) *Euterpe*, 104.
Hist. Afric. par. 1. sect. 3.

(b) *Hist. Virginia*, p. 116.

(c) *Leo*

but are no more to be found there of their original tawny Colour. The King of *Gualata* is supposed to be lineally descended from these tawny *Moors*, but is even blacker than the original Negroes (*a*). The *Habessines*, who came from *Arabia* originally (*b*), are no longer of their swarthy Complexion, but have got the black Complexion of the *Ethiopians*, whose Country they possess (*c*). The *Mosemleeks* of *Canada*, who wear Cloaths, and are more civilized than the other Savages their Neighbours, who go stark naked, are so much more refined in their Complexions by this Usage, as to be taken for *Spaniards*, and not *Indians* (*d*). Nay, the *Spaniards* themselves, who have inhabited *America* under the Torrid Zone, for any Time, are become as dark-coloured as our native *Indians* of *Virginia*, as I have been an Eye-witness: And were they not to intermarry with the *Europeans*, but lead the same rude and barbarous Lives with the *Indians*, it is very probable, that, in a Succession of many Generations, they would become as dark in Complexion.

(*a*) Moore's *Travels*, 214.
c. i. (*c*) *Idem*, l. i. c. 14.
lett. 16. vol. i.

(*b*) Ludolph. *Hist. Æthiop.* l. 1.
(*d*) La Hontan. *Nouv. Voyage*,

June 14. 1744. the SOCIETY adjourned to
October 25.

V. *A Letter from William Hallett, M. D. to Henry Pemberton, M. D. F. R. S. & Chem. Prof. Gresham; containing the Case of a Lad, who was shot through the Lungs; drawn up by Mr. Nich. Peters junior, Surgeon.*

To Dr. Pemberton.

Dear Sir, Exeter, Sept. 21. 1744.

*Read Nov. 8.
1744.*

I HAVE now sent you the remarkable Case, which I shew'd you lately when you were at my House, of a Lad who was shot in the Lungs. It is drawn up by Mr. *Peters*, his Surgeon; and you have it under, in his own Hand.

To Dr. Hallett, at Exeter.

S I R, Topsham, Sept. 12. 1744.

ACCORDING to your Request, I send you the Case of *James Channon*; which, as it was pretty remarkable from the Beginning, I then noted down; so that you have the Symptoms related not barely from Memory, but as they were in Fact when they occur'd. I am,

S I R,

Your most humble Servant,

Nich. Peters, junior.

Dec. 28. 1737. James Channon, aged about 14, was accidentally shot in his Back by another Lad,

U

at

at the Distance of two Yards from him; so that the whole Load of Shot, not having Space to scatter, enter'd like a Ball, by the Edge of the Left *Scapula*, which it splinter'd; and, slanting upward, pass'd between the two superior Ribs, and fractur'd the Clavicle; the Resistance of which Bone hinder'd their Passage thro' the Skin; for some of them lay immediately on the fractur'd Part, cover'd only by the *Cutis*; which, with a Touch of the Incision-Knife, I took out, in Number about a Dozen: They were the small Mustard-seed Shot.

After reducing the Fracture, I drew off ten Ounces of Blood (he having lost but very little by the Wound); and treated the Wound in his Back as usual in Gun-shot Wounds; and the Fever which attended it, as a common symptomatic Fever. In 8 or 9 Days time a plentiful Suppuration came on, and his Fever abated.

Towards the Middle of *January*, the Discharge of fetid *Pus* was so great, not only through the Wound, but also by Expectoration, that I thought he could not long survive it: At each Time of Dressing (which was Morning and Evening, till the Quantity lessen'd) full six Ounces of *Pus* were discharg'd: The like Quantity he would generally cough up between the Dressings. When the Dressings were removed, I frequently made him force a Cough, and try if he could not throw out any *Pus* by his Mouth; but, instead of passing that Way, it flew out thro' the Wound, like Water from a Pump: If I blocked up the Wound with Tow, he could then freely discharge it by the Mouth. When the Matter had done flowing, the Air which was forc'd thro' the Wound by Coughing, would blow out a Candle, which I often experienced.

experienced. The Matter was so prodigiously fetid, that, for some time after he was dress'd, the Stink in the Chamber was scarce tolerable; and it was near the Middle of *March* before the Discharge began to abate. In this Space of Time he cough'd up 25 Shot; had frequently hectic Heats, and Night-Sweats; a quick feverish Pulse return'd constantly towards Evening, with great Thirst; he had lost his Appetite, and was greatly emaciated; his chief Food was Milk, and Physick the Bark.

In order to give a free Passage to the Matter thro' the Wound, and prevent the Stink from killing him (as the poor Boy expressed it), by coming through the Mouth, I had for some time kept a *Cannula* in the Wound; but, in less than a Fortnight, I found myself obliged to leave it quite out; for tho' it answer'd the End of giving the Matter a free Passage that Way, and prevented its coming by the Mouth, yet the Quantity thro' the Wound increased daily, and his hectic Heats became more violent.

Seeing no Prospect of any End to be put to the Discharge of Matter, it keeping up to its usual Quantity for a Month or 6 Weeks longer, and the poor Boy reduced to a meer Skeleton, I was determin'd, if possible to heal the Wound, and commit the Event to Nature; there not being one favourable Symptom to give the least Hope of his Recovery.

About the Middle of *June* the Wound was quite cicatriz'd, 2 or 3 Exfoliations being first cast off from the *Scapula*. His Cough still continued with a Discharge of the same fetid Pus, but in three Weeks it began to abate; and, towards the latter End of *July*, he had gain'd Flesh, and his Cough had left him; he

walked abroad, and was, to Appearance, quite recover'd. But this fair Prospect did not last long; for, towards the latter End of *August*, I was called to him in the Night, and found him supported in the Bed, with a Half-pint Basen in his Hand, almost full of the same Sort of stinking putrid Matter, which he used to cough up: It had been emptied but a Quarter of an Hour before, so that, in less than half an Hour, he had expectorated a full Pint. This Cough continued upon him 16 Hours longer; when, the Load of Matter being pump'd up, he grew much better. Two or three Days before this severe Attack, he had complained of being faint, feverish, and strait at the Breast, for which he was bled, &c. In this Fit of Coughing, he brought up with the *Pus* 14 Shot. He had three of these violent Returns before the Summer was quite over, which reduced him nearly to his former weak State, but discharged no Shot.

In *November* following, I laid on a Caustic to the *Cicatrix* of the Wound in his Back; and kept it open with a large Bean, to try if a Discharge, by way of Issue, might divert the Matter from coming by the Mouth: He had no such violent Seizures afterward; but still a hectic Cough upon him, which expectorated a small Quantity of the same fetid *Pus*: The Discharge from the Issue was pretty considerable, and he weather'd out the Winter tolerable well.

In *March* 1739, he grew feverish, and complain'd of a great Load and Pain just above the Diaphragm, on the Left Side: I applied a warm Plaister, and drew off ten Ounces of Blood, which I found was pretty much inflamed. A few Days afterwards an Abscess formed between the Ribs, where he had before complained

plained of the Pain ; which I opened, and discharged about four Ounces of the same fetid Matter, and 18 Shot. Here was a true *Empyema*, and I had now great Hopes of a Cure, Nature having pointed out such a depending Part, for a Discharge of what Matter might be lodged in the *Thorax*. I then healed up the Issue in his Back, and kept this new Wound open with a *Cannula* ; but, within ten Days, the Matter had ceased flowing, his feverish Symptoms again increas'd, and his Cough return'd with a Discharge of the same putrid Matter. I threw aside the *Cannula*, and healed the Wound between the Ribs, it answering no End to keep it open longer. The remaining Part of the Year 1739, he had several Returns of his Cough, with pretty large Expectations, but they were not of long Continuance ; frequent Bleedings, a Milk-Diet, and vulnerary Medicines, were made use of.

In the Years 1740, 41, 42, towards Spring and Autumn, he generally was seized with a Difficulty of Breathing, and Pain of the Side: Bleeding would relieve him for the present, but it seldom ended without a Discharge of the same Sort of *Pus* by Coughing, and with it, sometimes, one or two Shot. At the latter End of the Summer 1741, he had an Abscess formed in the left Side, between the true and spurious Ribs: I open'd it, and with the Matter discharged 9 Shot.

Between these grand Fits of Coughing (if I may so call them), which happen'd three or four Times in the Year, he would gain Strength, grow fat, and work at his Trade of Glove-making.

Towards

Towards the latter End of *March* 1743, his Cough return'd again with the same usual Violence, and the Discharge in One Night was a full Pint of fetid bloody *Pus*; half that Quantity was expectorated next Day. He had the same feverish Symptoms a few Days before this Cough, as usual, but rather more severe. It continued upon him 8 Days before it began to abate. No Shot were discharged at this time, as was expected; but he coughed up a broad Scale of a Bone, ragged at the Edges, and of an irregular Shape; which, I imagine, was a Part of the *Scapula*. A few Weeks after this he was taken into your Hospital.

Thus far Mr. *Peters*. He had the Benefit of the *Devon* and *Exeter* Hospital, under my Care for 15 Months; during which Time he was hestical, had sometimes pleuritic Pains, for which he was often bled, and took soft Pectorals. He frequently spit *Pus* in great Quantities. I confin'd him to a Milk-Diet; order'd him Balsamics, particularly *Bals. Lacatelli* in an Electuary. He is now healthy, strong, and fat; and frequently walks from *Topsham* to *Exeter*, which is near four Miles, and returns the same Day.

I am &c.

William Hallett.

VI. *Extraēt of a Letter from Dr. John Bartram, to Mr. Peter Collinson, F. R. S. containing some Observations concerning the Salt-Marsh Muscle, the Oyſter-Banks, and the Freſh-Water Muscle, of Penſylvania.*

My good Friend,

Read Nov. 8. 1744. **I** HAVE obſerved ſomething of an extraordinary Nature in our *Salt-Marſh Muscle*: By its fibrous Roots, which ſtrike deep into the Soil, it ſeems to be of a vegetable Nature; for, it is highly probable, the Animal draws ſome Part of its Nouriſhment through them: They are fixed by theſe two Thirds of their Length in the Sand, with their broad Ends uppermoſt, which open at every Return of the Tide, to be replenished by the Salt-Water: When it is retreated, they are found lodged in the Graſs, Sedge, Creeks, and Banks, ſingly and together in Plenty.

I herewith ſend you a Specimen, which will give you a better Idea of this wonderful Creature.

There you may plainly obſerve the Ligaments draw their Origin from the principal Parts of the Animal, and unite near the Extremitie of the Shell, which they paſs through on that Side of the Muscle that opens to let in the Water; then they divide again into many capillary Roots or Fibres, which penetrate and extend themſelves into the Mud or Soil of the Marſh; which, by long Obſervation, ſeem to me for two Uſes; firſt, as I have above obſerved, to convey Part

Part of their Nourishment; which seems probable, by their being dispersed through the Body of the Muscle (This is better seen when alive; but now they are dry, one of the Specimens plainly shew it). See TAB. II. Fig. 1.

The other Use of these fibrous Roots (for so I must call them), by their striking deep into the Mud or Sand, is to secure the Creature from being carried away by the Rapidity of the Tide: So that, in this Circumstance, they are somewhat analogous to Plants, whose Roots both nourish them, and secure them from the Injuries of Wind and Flood.

OUR *Oysters* are of an oblong Figure; they grow at the Sides and Bottoms of Creeks, Rivers, and Bays, near the Sea; but mostly in such a Situation where they are near or quite dry at low Water: They have the Power of Opening and Shutting, like the Muscle, to take in and retain the Salt-Water, which is their principal Nourishment: Tho' they stick in the Mud, they are not so secured as the Salt-Marsh Muscle before-mention'd; and tho' these Oysters grow in great Clusters or Heaps, commonly called Oyster-Banks, yet every one that is alive hath free Communication with the Air and Water, and Liberty to open and shut. If the Oyster's Way of growing may be compared to that of a Plant, I think there is great Similitude between it and the *Opuntia*, or *Indian Fig*; a Leaf produces and supports a Leaf, and so on: Thus the young Oyster grows on the Sides of the old one, which, by degrees, is so deep immersed in the Mud, that it dieth; but yet it serves to support the young one upright, until it comes to Maturity to produce others; and then that, by degrees, subsides; so that, by this Method, Banks of dead and living Oysters are extended of an inconceivable Length and Breadth through all our Coasts.

Our

Fig. 1. p. 158.

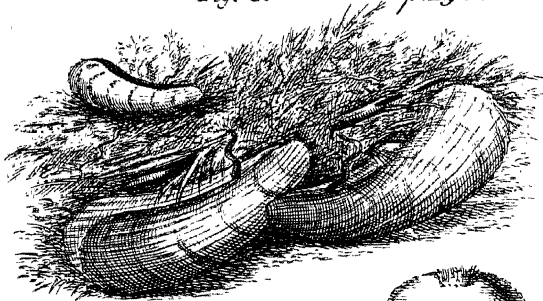


Fig. 11. p. 238.

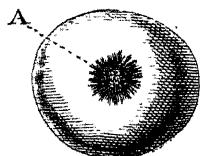


Fig. 12. p. 238.

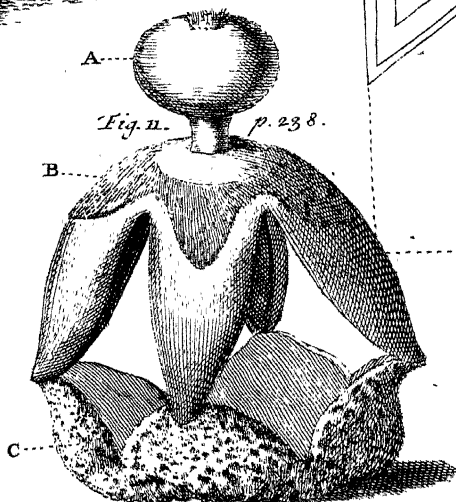


Fig. 8. p. 195. 200.



Fig. 2. p. 160.

IMP·CAES·M·AVRELIO
SEVERO·ANTONINO
PIO·FELICI·XXG·PARTHIC·
MAX·BRIT·MAX·GERM·
MAX·PONTIFICI·MAXIM·
TRIB·POTEST·XVIII·IMP·II·
COS·III·PROCOS·PP·CO·I·
FIDA·VARDVL·CREO·ANO
NNANA·FECIT·SVB·CVRATO
LEG·XX·GR

Fig. 3. p. 160.

MARTI
VICTORI
VLIVS LI
IVS TRIB
V·S·L·M·

Fig. 4. p. 160.

FORTVNAE
AVG
A E T
PROCVLINA
V S

Fig. 10. p. 208.

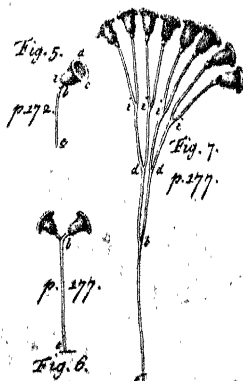
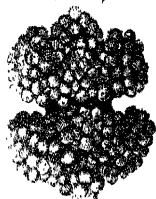


Fig. 9 p. 201.

DEO·HER
SAEG·O·N
TTAMMON
SAEN·TAMMON
VITALIS
HONC

OUR common *Fresh-Water Muscles* differ from our Salt-Marsh Muscle, in that they are not fixed to any Place or Thing, but have a Method of trailing along on the sandy Bottoms of Creeks and Rivers: They have the Power not only of opening and shutting their Shells at Pleasure, but have, moreover, the Power of creeping (*a*) along as it were like a Snail, by turning upon the upper Edge that opens, and so work themselves along the soft yielding Sand in little Furrows about half an Inch deep. I have traced them for several Yards, by these little Channells, when the Tide is down, and left the Sands bare.

If these few Observations prove acceptable, it will be a Pleasure to

Your Friend,

John Bartram.

VII. *A Letter from Mr. Christ. Hunter, to Dr. Mortimer, Secr. R. S. serving to accompany a Copy of an antient Roman Inscription at Rochester in Northumberland, and two others at Rissingham.*

Worthy Sir,

Durham, Sept. 9. 1744.

*Read Nov. 8.
1744.*

KIND Providence enabling me to outlive such Members of the *Royal Society* I have had the Happiness to correspond withal,
Dr.

(*a*) I have seen this of our Horse Muscles in Ponds here in *England*.

Dr. *Lister*, Dr. *Woodward*, and the much respected dear Mr. *Gale*, I make bold to address the following Inscriptions to the honourable *Society* by your Hands.

A Transcript of the first Inscription, sent to me six Months ago, was copied by a Person of too narrow Judgment to be depended upon; which obliged me to take a Journey to *Rocheſter*, to be exact in every Circumſtance relating to the ſame (a): The Stone was found erected upon two Supporters, and diſcovered laſt Year in the miſt of a Heap of Rubbiſh, and the Inſcription intire to the laſt Line, where the Name of the *Præfectus* is viſibly obliterated, in ſome enſuing Reign.

The two following (b) I took out of the Earth at *Riſingham*, another *Roman* Fort, eight Miles South from *Rocheſter* upon the Banks of *Reed's* Water mentioned by Mr. *Camden*: They are both Altars broken, the firſt falling from the Top of the higheſt Wall of a Dwelling-houſe deſerted above fifty Years; the two initial Letters of the third and fourth Lines broken off; as *IVLIUS, LIVIVS, LIBIVS, LICIVS, or LIDIVS*. The other is upon the Margin of Mr. *Warburton's* Map of *Northumberland*, I fear, omitted by Mr. *Horſley* in *Britannia Romana* among his Sculptures; but find it in pag. 240. of his *Observations*, ſo quite different from my Copy. I am,

Good Sir,

Your moſt obedient humble Servant,

Chriſtopher Hunter.

(a) See TAB. II. Fig. 2.

(b) See TAB. II. Fig. 3 and 4.
VIII.

VIII. *An Account of some Magnetical Experiments, shewed before the Royal Society, by Mr. Gowan Knight, on Thursday the 15th of November, 1744.*

Read Nov. 15.
1744.

MR. Knight, of *Magdalen-College* in *Oxford*, being introduced to a Meeting of the *Royal Society* on *Thursday* the 15th of *November*, 1744, produced, before the Gentlemen there present, several curious artificial Magnets contrived by himself; some of which consisted of plain Bars of Steel naked, and other of Bars or Blocks of the same Substance, armed with Iron after the common manner of natural Loadstones: But, as he was apprehensive the Trials he had before made of the Weights these Magnets were respectively capable of lifting, could hardly be repeated with sufficient Exactness and Advantage before so large a Company, he desired to refer himself, for those Particulars, to what the *President* of the *Society* had seen at his Lodgings on *Wednesday* the 7th, and on *Tuesday* the 13th, of the same Month of *November*.

Whereupon the President acquainted the Company, that he had lately been several times at Mr. Knight's Lodgings, where he had seen many Experiments made with his artificial Magnets; and that, particularly on the Days above-mentioned, he had been present, and had taken Minutes of the following Trials then made by that Gentleman; by which it appeared, that,

A small eight-corner'd Bar of Steel, of the Length of 3 Inches, and almost 7 Tenths, and of the Weight of about half an Ounce *Troy*, lifted by one of its Ends about 11 of the same Ounces.

That another plain Bar of Steel of a Parallelopiped Form, of the Length of 5 Inches and 9 Tenths, the Breadth of four Tenths, and the Thickness of two Tenths of an Inch, weighing 2 Ounces 8 Pennyweight and a half, lifted, in like manner, by one of its Ends 20 *Troy* Ounces.

That a Steel Bar, almost of the same Form as the last, but only 4 Inches in Length, capped or armed with Iron at each End, cramped with Silver, and weighing all together one Ounce fourteen Pennyweight, lifted by the Feet of the Armour full four Pounds *Troy*.

That a single Block of Steel of a Parallelopiped Form, almost 4 Inches long, 1 Inch and 2 Tenths in Height, and 4 Tenths of an Inch in Thickness, armed with Iron, cramped with Brass, suspended by a Ring of the same, and weighing all together 14 Ounces 1 Pennyweight, lifted by the Feet of the Armour 14 Pounds 2 Ounces and an half, *Troy* Weight.

That a compound artificial Magnet was also tried, consisting of 12 Bars of Steel armed; and that it was found to lift by the Feet of the Armour as the last, 23 *Troy* Pounds, 2 Ounces and an half.

The 12 Bars, composing this last Magnet, were each a little more than 4 Inches long, 3 Tenths of an Inch in Breadth, and 16 Hundredths of the same in Depth, weighing one with another about 25 Pennyweight each. They were all placed one upon another

other, so as to make together one Parallelopiped Body, of the common Length and Breadth of the several Bars, but of the Height of near 2 Inches, being the Sum of the respective Thicknesses of all the Bars taken together : And this Parallelopiped Body, being cramped with Brass, and fitted with an Handle of the same Metal, was armed at the 2 Ends that were made up of the common Extremities of all the Bars, with 2 substantial Pieces of Iron, after the common manner of arming natural Loadstones, the whole Frame weighing together about 20 *Troy* Ounces.

Besides these, the President made also the following Report of some Trials he had seen made at the same time of the Effects of an Art Mr. *Knight* is Master of, by which he can improve or increase the lifting Powers of natural Loadstones.

He carried with him, on *Wednesday* the 7th of *November*, a small armed Loadstone belonging to an Acquaintance, which weigh'd, with its Armour, 7 Pennyweight 14 Grains ; but which, being reputed but of an ungenerous Nature, took up, and with some Difficulty, barely 2 Ounces. Mr. *Knight* took it into his Study, and, returning it in about a Minute, it then took up better than 4 Ounces with Ease : But, upon his saying, it would still gain some more Strength, by remaining with him some time, it was left till the 13th, when it took up distinctly, with the same Apparatus as before, 6 Ounces 18 Penny-weights and 3 Grains ; since which time it has also several times been found to lift nearly the same Quantity.

Mr. *Knight* further, at the same time, shew'd the President the following Instances of his Ability to invert

invert or change the Direction of the Poles in natural Loadstones.

Such a Stone belonging to Mr. *Francis Hauksbee*, weighing about 5 Ounces and 14 Pennyweights, of an irregular cylindrical Form, with 2 of the Sides somewhat flatted, upon which Armour had formerly been applied, had the Direction of its Polarity from one of these flatted Sides to the other, notwithstanding the Stone had a distinct Grain running at right Angles to that Direction. It was tried and observed, that one of these flatted Sides strongly attracted the North End, and repelled the South; and that the other attracted the South, and repelled the North End of the magnetic Needle. The End of the Stone, attracting the South End of the Needle, was then marked, by the rubbing of a Piece of Silver upon it, as upon a Touchstone: After which, Mr. *Knight* carried the Stone into his Study; and, re-producing it in about a Minute, shew'd, that the Poles were then directly inverted; and that the same End, which before attracted the South End of the Needle, now attracted the North, and repelled the South, and *vice versa*.

After this, Mr. *Knight*, again taking the Stone, brought it back in as short a time as before, with the Direction of its Polarity turned at right Angles to its former Direction, and into the Direction of the natural Grain of the Stone, the Poles now lying in the flat Ends of the Cylinder; one of which, being the smoother End, attracted the South End of the Needle, whilst the other, which was of a rougher Texture, attracted the North End, and repelled the South End of the same: When it was also observed, that the Polarity

appeared stronger in this Case, than either of the former.

Lastly, Mr. *Knight*, in about the same time, inverted this last Direction of the Poles, keeping it still parallel to the Axis of the Cylinder, but causing the smooth End of the Stone to attract the North End of the magnetic Needle, and the rough End to attract the South, and repel the North End of the same Needle.

After this Report, Mr. *Knight* proceeded to shew, at the Meeting, some of the same artificial Magnets therein mentioned; and it was found, that the compound Magnet, consisting of 12 Steel-Bars, and which had, in the Experiment made before the President, lifted 23 Pounds 2 Ounces and an half *Troy* Weight, did here, under all the Inconveniencies and Disadvantages of a crowded Room, still lift a Weight amounting to 21 Pounds and 11 of the same Ounces.

It was also found, that the single armed Block of Steel, which had before lifted 14 Pounds and 2 Ounces, did here, under the same Disadvantages as the former, lift 13 Pounds and 7 Ounces of like *Troy* Weight.

And, lastly, Mr. *Knight* produced to the Company the above-mentioned natural Loadstone belonging to Mr. *Hawksbee*, but with the Direction of its Polarity again altered from what it was, when it was last seen by the President.

P. S. Since the artificial Magnets mentioned in the foregoing Paper, Mr. *Knight* has caused some others to be made of a lesser Size, but of a very great lifting Power: And one of these, weighing

weighing without its Armour just an Ounce, and with the Armour, Cramps, and Rings, 1 Ounce 17 Pennyweights, lifted, before the President of the *Society*, on *Friday* the 27th of *July* 1745, 6 Pounds and 10 Ounces *Troy* Weight.

This Magnet consisted of 3 Plates of Steel, each 2 Inches long, 7 Tenths of an Inch in Breadth, and not above 6 Hundreths of an Inch in Thickness: They were laid flat upon each other, and screwed together by 2 small Brass Screws going through the 3 Plates. After which, the little Parallelopiped Block so made up, was armed with Iron at the 2 Ends, cramped together with Silver, and fitted with a double Ring of the same Metal, for the convenient holding of it.

IX. *Abstract of what is contained in a Book concerning Electricity, just published at Leipzic, 1744. by John Henry Wintler, Greek and Latin Professor there; from Article 75 to Article 79.*

Read Nov. 22. 1744. **T**HE electrical Sparks from Metals, such as Iron and Silver, are capable of kindling all such Fluids as may be otherwise kindled by actual Flame. And this Experiment succeeds best, when the *quinta Essentia vegetabilis* is held in a Spoon under the Cross of a Sword, whose Point is turned towards the electrifying Glass (TAB. II. Fig. 4.). In like manner, the same Spirits may easily be set on Fire,

Fire, by the Sparks proceeding from an electrified Tube of Tin.

This Experiment with the Sparks coming from Metals when made electric, was first made by Dr. *Ludolph*, of *Berlin*; who, toward the Beginning of the present Year 1744, kindled, with the Sparks excited by the Friction of a Glass Tube, the *ethereal Spirits* of *Frobenius*. This was done at the Opening of the *Royal Academy*, and in the Presence of some Hundreds of Persons. This Account was not only related in the *Berlin Gazette*, of the 30th of *May* last; but has been since confirmed by several Letters, sent from *Berlin* to *Leipsic*, to Count *Mantoufet*, immediately after the Experiment.

Mr. *Marshall*, who now studies here, also communicated to me a Letter he had received from *Berlin* concerning the same; and I have since been also certified of it, by the Account of several Men of Learning, that had seen the Experiment at *Berlin*, and that have since visited me at this Place. Lastly, Mr. *Reinhart*, who came hither about last *Easter*, with Count *Zaluski*, Great Chancellor of *Poland*, told me, that the Experiment was not difficult to be made; and that the Liquor, called *Quinta Essentia* * *vegetabilis*, might very readily be kindled by the electrical Sparks. I immediately sent for some of that Effence, and found the Experiment succeed to my Wish.

Red-hot Iron sets no Spirits on Fire, tho' held very near to those Spirits; but if that Iron is made electric, its electric Sparks very readily kindle all well-rectified Spirits.

The

* i. e. Spirit of Wine so highly rectified, as, being pour'd upon Gunpowder, and then being set on fire, will at last flash the Gunpowder.

The Sparks that proceed from the Body of a Man, made electrical, kindle Spirits as quick as those from electrified Metal, whether the Body of the Man is rendered electric immediately by the Glass Tube, or by the intermediate Tube of Tin.

I made this Experiment with Success upon myself, before his Excellency Count *Manteufet*, at his House, about the middle of last *May*, in the Presence of Professor *Christian Wolf*, of *Hall*, and many others. Neither myself, nor any of the Company, knew, at that time, that the electric Sparks, from the Body of a Man, were capable of kindling Spirits; but, upon seeing the *Quinta Essentia vegetabilis* kindled with extraordinary Quickness, by the Sparks proceeding from an Iron Tube that was rusty, one of the Company started the Question, Whether the Sparks, from the Body of a Man, might not possibly do the same? Upon which I immediately stepped on to a Frame, over which blue silken Lines were extended: I took hold with one Hand of the rusty Iron Tube, and held the Fingers of the other over some of the *Quinta Essentia*; and the Sparks from my Fingers immediately struck with such Violence into the silver Spoon that held it, that the Essence was in a Moment set all in a Flame.

This Experiment, so unexpected, gave the greatest Satisfaction to all the Company; and an Account of it was published in the *Leipsc Gazette* of the 21st of *May*; where it was also mention'd, that divers other Experiments, with the Sparks of electrified Metal, had already been made both at *Dantzic*, and at *Berlin*.

Dead Fowls, Pork, and Veal, both raw and drest, may be made electric by a Tin Tube, or by the Hand

Hand of a Man ; infomuch that the Sparks, proceeding from those feveral Bodies, will alfo kindle the fame Effence.

If fuch fluid Bodies, as are ufually kindled by Flame, are not fine enough, they need only be warm'd a little in the Spoon : Or the Spirits may be lighted a little before, and blown out again, before they are brought to the electrical Body.

In this manner I have kindled, with the electrical Sparks, camphorated Spirits of Wine, coloured with Saffron, the common *Effentia vegetabilis* ; and even French Brandy, and Corn-Spirits, only taking the Precaution of warming thefe Liquors a little before.

Even Oil, Pitch, and Sealing-wax, may be lighted by the electric Sparks, provided they are before heated to a Degree that is next to kindling.

X. *Translation of a Letter from Mr. Abraham Trembley, F. R. S. to the Prefident, with Observations upon feveral newly discover'd Species of Fresh-water Polypi.*

S I R,

Sorgu'iet, 6. Nov. 1744. N. S.

Read Nov. 22.
1744.

I HAVE herewith the Honour of transmitting to you the Particulars of feveral Observations I have made, during the Courfe of the laft Summer, upon fome Species of very minute Water-Animals ; and which are the fame I have already made fome Mention of, in the third Paragraph of the 297th Page of the *Memoires pour servir à l'Histoire des Polypes à Bras en forme*
de

de Cornes. Mr. *De Reaumur* judges them all to belong to the general Class of the *Polypi*; and he has already distinguished and distributed the several Species of them; to which he has given the respective Names that I have made use of in the inclosed Abstract of my Observations.

I am very sensible, that some Passages of this Account will hardly appear sufficiently intelligible, to such as have not yet taken Notice of the little Creatures I am speaking about: But this Inconvenience I could not entirely have avoided, without entering into too particular a Detail of Facts, which I have not yet prosecuted so far as I intend, and which I could not besides have thoroughly explained, without the Assistance of a great Number of Figures.

I hope, notwithstanding, that what I have said will abundantly shew how singular the *Animalcula* in Question are, and how well they deserve Notice and Consideration. I shall neglect no Opportunities of pursuing their History; thereby to enable myself hereafter to give a more perfect Account of what I shall have further learned in my Enquiries. But this cannot be done immediately, as a considerable Time is required, for the making of accurate, repeated, varied, and well-connected Experiments.

In the mean time, as I have always made it my Pleasure, I shall even look upon it as a Duty, to be at all Times ready to satisfy the Curiosity of such as are Lovers of Natural History, with regard to the several Particulars that may present themselves, and that I may think worthy of their Attention.

I shall

I shall only add further, that I have already communicated these Observations to several Persons of the greatest Knowledge and Distinction, who have been pleased attentively to examine the same, and thereby given me the Satisfaction of having the best and most unexceptionable Witnesses to all the principal Facts that are mention'd in the inclosed Account; which I now put into your Hands, and remain, with the truest Respect,

S I R,

Your most humble,

and most obedient Servant,

A. Trembley.

WE find, in divers Places, upon Water-Plants, and other Bodies in the Water, a whitish Substance, that looks at first only like a sort of Mould: We sometimes see Plants, Sticks of Wood, Snail-shells, and the like, that are entirely covered over with this Substance. But if we take any of these, put them into a Glass of clear Water, and then examine with a magnifying Glass what is upon them, we soon discover, in the little Bodies, that, by their Assemblage, form this whitish Substance, such Motions as give sufficient Reason to look upon them as living Animals; and this will appear yet more sensible, when they come to be observed with a Microscope. We then find them to be minute Bodies, severally fixed to the Extremities of small Stems, or Pedicles, many

many of which are often so united, as to form together a Sort of Branches, or *Clusters*; and this Sort of Appearance determined Monsieur *De Reaumur* to name the *Animalcula* that appeared so fixed, *clustering Polypi: des Polypes en bouquet*.

These *Clusters* are larger or lesser, according to the Species of the *Polypi* that form them, and according to the Concurrence of many other Circumstances.

To get a clear Idea of the Figure of these Animals, it is best to observe the smaller *Clusters*; as, in the larger, the great Number of the *Polypi* upon the several Stems, are apt to hide one another.

There is a Case, I shall mention presently, where the *Polypi* are single; and it is proper to observe them in that Case; and the rather, because that is the Way to discover how the *Clusters* are formed.

I shall now describe one of these single *Polypi*, to give a general Idea of the Form of the Animal: and I shall herein chiefly endeavour the Description of that Species which I have most particularly made my Observations upon.

These are not in Length above the 240th Part of an Inch, and are of a Shape nearly resembling that of a Bell: this may be seen in the Figure, where one of them is represented exceedingly magnified (TAB. II. Fig. 5.). The anterior Part, *ac*, generally appears open, when it properly presents itself; the posterior Part *b* is fixed to a Stem or Pedicle *be*; and it is by the Extremity *e* of this Pedicle, that the *Polypus* fastens itself to any other Sort of Body. The *Polypus* of this Sort generally appears to the Microscope of a brownish Colour, excepting at its smaller End *b*, where it is transparent, as well as its Pedicle *be*. When the anterior Part *ac* is open, one may perceive

ceive about its Edges a very lively Motion ; and when the *Polypus* presents itself in a certain manner, it discovers, on either Side of these Edges of its anterior Part, somewhat very much resembling the Wheels of a little Mill, that move with great Velocity.

These *Polypi* are able to contract themselves ; and they do so often, and suddenly. They may be brought to contract at any time, either by touching them, or by moving the Body to which they are fixed. When they contract, the Edges of their anterior Parts are drawn quite into their Bodies ; and when they resume, which they do soon after, their former Posture, one may distinctly see those Edges come forth again, and put themselves in Motion, as before.

When one looks about the anterior Parts of these *Polypi*, which are open, and whose Edges are in Motion, one may frequently have an Opportunity of remarking a Number of very minute Bodies swimming in the Water, that seem to be forced down with Velocity into these Openings of their anterior Parts, and that sometimes are thrown out again from thence.

To make this Observation the most sensible, it is best not to look at a single *Polypus*, but a Cluster of some Numbers of them together.

I have taken Notice, that the *Polypi* of the Sort in Question, appear of a brownish Colour when viewed with the Microscope ; I should now add, that having left some of them for several Days in the same Water, they by degrees lost their brown Colour, and became transparent ; excepting only that a few Grains or Spots of Brown or Black, still continued

nued to be discernible in their Bodies : But, having afterwards removed these *Polypi* into other Water, newly taken out a Ditch, they in a little time resumed the same brown Hue which they had before.

It may commonly be observed, that when the *Polypi* are in Water newly put to them, there fall upon their anterior Parts far greater Numbers of the above-mentioned minute Bodies, than when they have been left for any time in the same Water.

It is very probable, that these minute Bodies are exceedingly small Animalcules, upon which the *Polypi* feed ; and that, consequently, the Opening which they have in their anterior Part, serves them for the Purposes of a Mouth.

The *Polypi* that have become transparent, and that have been left some time without the Addition of such Water as would make them recover their brown Colour, have also, at the same time, left off multiplying. But I have observed that others of them, to which I have afterwards given new Water from the Ditch, have soon after begun to multiply again.

These *Polypi* are capable of swimming about ; and when they swim, they are no longer in Clusters, but always single ; and they do not then appear in the same Form as when they are fixed, and open at their anterior Ends. It is by Swimming that they leave the Place to which they first appeared fixed, and that they go and fix themselves to any other Body that they find in their Way.

One should begin to observe a *Polypus* soon after it has fixed itself singly, in order to see regularly in what manner the Clusters form themselves, and in what Way these small Creatures multiply.

The

The Stem or Pedicle of a *Polypus* that is yet single, and which has but lately fixed itself, is at first very short, but it lengthens itself in a little time. After that, the *Polypus* multiplies; that is to say, it divides or splits itself into two lengthwise. One first observes the Lips to be drawn into the Body, whose anterior Part closes, and becomes round: the Motion that was to be seen before the Lips were drawn in, no longer appears; yet may one see, by looking with Attention, a slow Motion within the Body, during all the Time that the *Polypus* remains closed. The anterior Part of the *Polypus* flats itself afterwards by degrees, and spreads in proportion, becoming broader as it shortens; it then gradually splits down through the Middle, that is, from the Middle of the Head to the Place where the posterior End joins to the Pedicle: so that, in a little while, there appear two separate round Bodies joined to the Extremity of the Pedicle that just before supported but one.

The anterior Part of each of these Bodies then opens by degrees; and, as they open, the Lips of the new *Polypi* shew themselves more and more. Then is the Time of observing these Lips with Attention, and of forming to one's Self an Idea of their true Form, and of their Motion already spoken of. This Motion is at the first very slow, it quickens as the *Polypi* continue to open; and, as soon as they have done, it becomes as swift as that which appeared in the Lips of the whole single *Polypus*, before it began to divide: and then these new *Polypi* may be looked upon as entirely formed.

They are, at first, less than the *Polypus* from which they were formed ; but they grow to the same Size in a very little time.

A *Polypus* is an Hour, or thereabout, dividing itself.

To have a tolerable Notion of this Operation, one must have seen it divers times, and in *Polypi* placed and situated in divers different Ways.

The Lips of these *Polypi* appear to be composed of four or five transparent Stripes, all which have an undulating Motion. Whilst the *Polypi* are opening, and that the Motion of their Lips is yet but slow ; one sees on either Side, when they are in a certain Position, what one is inclined to take for the Wheels of a Mill, in the *Polypi* that are quite formed, and whose Lips move very fast : but one now sees, while they are opening I say, what may be rather taken for four or five Fingers on either Side of their Mouths ; which alternately bend down and extend themselves every Instant, and to which the transparent Stripes above-mentioned appear to be fixed.

This should be observed often, and in as many various Attitudes as possible, to avoid being deceived by the taking of Appearances for Realities ; which happens more or less to every Observer, more especially when he first begins to observe. Before I venture to explain myself more particularly upon this last Article, I shall endeavour to repeat and to pursue further, if I am able, some Observations that I have at present only begun.

When the first *Polypus* is thus divided, and the two new ones produced by this Operation are thus completed ; one sees on one Pedicle two *Polypi*, joined
to

to its Extremity by their posterior Ends, and that shew themselves on the Sides of each other, as in TAB. II. *Fig. 6.*

The ordinary Proportion between the Length of the Body of one of these *Polypi*, and the Length of their Pedicle, is pretty exactly observed in the Drawings.

Soon after the Separation is completed, each of the new *Polypi* begins to shew a Pedicle of its own.

I have often had Occasion to take notice, that each of the new *Polypi* had, the Day after their Separation, a Pedicle of a tolerable Length; and that these new Pedicles united at the Extremity of the first Pedicle, as the Branches of a Tree unite at its Trunk.

Several of the *Polypi*, upon which I have made continued Observations, have multiplied at the latest 24 Hours after their first Separation. The new Cluster has then consisted of 4 *Polypi*, each of which had its own Pedicle; as every one has also had, that was afterwards produced by a new Separation.

The next Figure represents a Cluster of eight *Polypi*; and by this Figure it may be apprehended in what manner the Pedicles of the *Polypi* become disposed, as their Numbers increase. These several Pedicles become so many Branches of the Cluster or Sprig. TAB. II. *Fig. 7.*

This Figure particularly represents a Cluster, whose Progress I followed in the Month of *September* last, 1744. It consisted, on the 9th Day of that Month, but of one single *Polypus*, which was placed as at *b*: this *Polypus* divided itself that Evening, and at half an Hour after Eight of the Clock, there were to be

discovered at *b* two perfect *Polypi*; whose Pedicles or Branches, *bd*, *bd*, continued lengthening till the Morning of the next Day, being the 10th of the same Month of *September*: at about a Quarter after Nine that Morning, these two *Polypi*, which were then at *d*, *d*, began also each to divide; so that at a Quarter past Eleven, there were at *d* and *d* four compleat *Polypi*, whose several Pedicles *di*, *di*, *di*, *di*, formed themselves soon after. On the 11th of the same *September*, about half an Hour after Seven in the Morning, I found that these four last *Polypi* had already again divided themselves; that is to say, that there were at *i*, *i*, *i*, *i*, eight distinct *Polypi*; and this Cluster, so consisting of eight *Polypi*, is here represented as it appeared upon the 12th of the same Month, between Ten and Eleven in the Forenoon.

The *Polypi* are not always ranged as they are disposed in this Figure; for it often happens, that the Pedicles and the *Polypi* are behind one another, so as to form a Groupe, in which some of the *Polypi* may chance to be hidden or covered by others, either entirely, or in Part.

This Figure represents the *Polypi* and Pedicles as magnified to the same Degree as those already exhibited in the former Figures.

I have taken notice of Clusters, the Numbers of whose *Polypi* have constantly gone on doubling, from 2 to 4, from 4 to 8, from 8 to 16, from 16 to 32; after which I have no longer been able to count exactly the Number of the *Polypi*.

I have said enough, to shew how the Clusters are formed, and how fast these small Animals multiply:
Indeed.

Indeed the Number is prodigious of those that are sometimes found in the Water.

I have large Glasses by me at this time, *Nov. 1. 1744. N. S.* in which they have exceedingly multiplied; there is particularly, in one of them, a Cluster composed of several lesser united Clusters, which is above an Inch over every way.

These detach themselves from time to time single *Polypi*, which go swimming about till they fix each upon some Body or other; and from these there again arise new Clusters, in the manner above spoken of.

The Branches, from which *Polypi* have detached themselves, still remain fixed to the Cluster, but they bear no more *Polypi*; and after all the *Polypi* of a Cluster have thus detached themselves from it, the Assemblage of the Branches still subsists, but is of no further Use.

I know of four other Species of *Polypi*, that all increase in the same manner as those I have been already speaking of; that is to say, which split and divide themselves according to their Length.

Those which come the nearest to the first, are somewhat more slender, and the Branches of their Clusters are transparent; yet do they appear, when there is a Number of them together, of a changeable Violet-Colour: the Clusters of these bear a good Resemblance to a Sprig or *Aigrette* of spun Glass.

When these last Animals are compleatly formed, it is not so easy to see distinctly in them the Motion of their Lips, as it is in the other Species before-mentioned; yet may it be observed in these also, whilst they are still opening, and compleating their Formation: for at such times, this Motion is

but

but slow, whereas it becomes afterwards very quick in those that are entirely perfected.

The *Polypi* of the other Species that I have observed, are yet less than the last: they are shorter, but more open and hollow'd at their anterior Ends. These have a Character that sufficiently distinguishes them from all the other Species: their Stems and Branches have a Motion that is not to be found in those of the other *Polypi*. These Stems draw themselves up, and shorten all at once, taking the Form of a spiral Wire or Screw; and a Moment after they again resume their former Shape, stretching themselves out strait as before.

These several Species of *Polypi* I have been speaking of, all multiply in vast Abundance; but they have also Enemies that destroy immense Numbers of them, and that in a very little time.

I have also this Summer observed regularly other small *Polypi*, of a different Sort from those that are found in Clusters. These are nearly in Shape like a Tunnel, pretty long in proportion to the Opening of their larger Ends. For this Reason, Mr. *De Reaumur* has thought proper to distinguish them by the Name of *Tunnel-like Polypi*.

I am acquainted with three Species of these last *Polypi*, which are respectively, green, blue, and white.

These must also be observed often, and in various Attitudes, in order to obtain a tolerably exact Notion of their proper Structure.

Their anterior End particularly, is of a far more compounded Shape, than one would at first imagine.

There

There may be discovered, round the Edges of this Part, a sensible Motion, much resembling that of an indented Wheel, or rather of an endless Screw, that is turned very fast about.

These *Tunnel-like Polypi* form no Clusters, like the others.

I have remarked, that the little Bodies, that pass swimming near the anterior Parts of these Insects, are in some manner drawn into the Mouths of their Tunnels; and I have sometimes seen a considerable Number of very small round *Animalcula* fall one after another into these Openings. Some of these were indeed afterwards let out again, at another Opening, which I am not yet able particularly to describe: but I could plainly see, that many of these little round Bodies remain'd within the Bodies of the *Polypi*; and it is therefore apparent, that these little Bodies, so taken in, became their Food.

These *Tunnel-like Polypi* do also multiply by dividing themselves into two, but they divide themselves otherwise than the *clustering Polypi*: they neither divide longitudinally nor transversely, but sloping and diagonal-wise. Of two *Tunnel-like Polypi*, just produced by the Division of one, the first has the old Head and a new posterior End; and the other the old posterior End, with a new Head.

I shall call that which has the old Head, the *superior Polypus*; and that which has the new Head, the *inferior* one.

The first Particulars observable in a *Tunnel-like Polypus* that is going to divide, are the Lips of the inferior *Polypus*; I mean those transparent Edges that are so conspicuous in the *Polypi* when entirely formed. These new Lips first discover themselves upon the
Poly-

Polypus that is going to divide, from a little below the old Lips, to about two Thirds of the Length of the *Polypus*, reckoning from the Head : but these new Lips are not dispos'd in a strait Line, according to the Length of the *Polypus*, but run sloping near half-way round about. These Lips are known by the Motion in them, but which Motion is at first very slow. That Portion of the Body of the *Polypus*, that is bounded by these new Lips, then gathers up itself, the new Lips insensibly draw together and close ; whereby there forms itself, at the Side of the *Polypus*, a Swelling, that is soon found to be the Head of the new one, bounded by the new Lips first discover'd. Before this Swelling is grown very remarkable, one begins to distinguish the two *Polypi* which are forming themselves ; and when that Swelling is considerably increased, the two *Polypi* will be discovered, no longer joined but by a small Portion to each other. The *superior Polypus* no longer adheres to the inferior one, but by its posterior Extremity, which is still fixed on one Side of the *inferior Polypus*. The *superior Polypus* then begins to make Motions that seemingly tend to the separating of him from the other ; and in a little time he becomes quite detach'd, swims away, and fixes himself elsewhere. I have seen one come and fix at the Side of the *inferior Polypus*, from which he was just before separated. The *inferior Polypus* remains fixed in the same Place, where the *Polypus* was that is now divided, and of which he was only the Half, before the Division took place.

I am not, at present, able to enter into a further Detail of the Manner in which these *Tunnel-like Polypi* divide and multiply themselves. I could not
do

do it, without the Assistance of many Figures, nor without the Mention of several other Facts, that I have not yet sufficiently satisfied myself about, nor observed so often as I think it necessary to do.

I shall also endeavour to carry on further the Natural History of all the several *Polypi* of which I have yet spoken, and, perhaps, that of some other Sorts besides; as I find, that the Experiments I make upon Insects of one Species, facilitate in several Respects those I have to make upon others; and that these last often throw a new Light upon Observations and Experiments already made.

As all these little Animals are exceedingly minute, I have hardly been able to observe any of the several Facts above-mentioned without the Assistance of the *Microscope*; but, if I was to take such small Objects out of the Water, in order to expose them to my Glasses in the common Way, I should both risque the losing of them, and hazard the putting them out of a Condition of performing their natural Operations. I am therefore forced to observe them with the Magnifiers of my Microscope, without taking them out of the Glasses I keep them in. I, for this Purpose, contrive to get them so near the Sides of those Glasses, that the *Foci* of my Magnifiers may reach them from without: I then fix, by the Sides of my Glasses, a jointed Arm I have fitted for that Purpose, into the Socket of which I can conveniently screw the different Magnifiers of my Microscope, and retain them fixed at their due Distances, by which I am able, with great Ease, to keep the *Animalcula* in Sight as long as I have Occasion for them: and I use for the most part the Light of a wax Taper, to illuminate my Objects.

XI. *Some Observations relating to vegetable Seeds*; by James Parsons, M. D. F. R. S.

Read Nov. 22.
1744.

AMONG the many Subjects in the Store-house of Nature, nothing, perhaps, is more entertaining, nor merits more the Attention of the Learned and Curious, than the Family of vegetable Seeds; and it is indeed surprising, that, till now, they have not been made an express Subject for the Consideration of some curious Naturalist, since Plants and Flowers have been treated of by so great a Number of Authors from the earliest Times.

It has been said lately, that the major Part of all the Seeds in the World are no more than minute *Molecules*; and so much alike, that little can be expected from them; whereas Plants and Flowers, being visible and beautiful, might be thought more worthy of Description and Delineation: But I should imagine, that those Parts of Nature which are least visible, and most obscure, require most to be inspected and explain'd, in order to render the Knowledge of them more general.

Pliny wisely says, "*Rerum natura nusquam magis quam in minimis tota sit*;" and, indeed, nothing is more true than that Assertion; for every Day's Experience confirms it: And altho' an *Elephant*, or *Camel*, has *Organs*, which, from heir noble Structure and Uses, are very wonderful, yet there is much greater Room for Admiration among us, that a Mite should be furnish'd with a greater Number of Limbs than so con-

siderable an Animal; since all our Knowledge is relatively conducted; and because there is something extremely perplexed and intricate in our Notions of Minuteness.

Such Reflections as these prompted me to examine several small Seeds; especially some of those, which, to the naked Eye, seem'd most like one another; and, to my great Surprise, I found them as different from each other in their Marks and Forms, as different *Genus's* of any other Class of the Creation; and, from their curious Characters and Beauties, I could not but conclude them as well worth observing and describing as any other Subject whatsoever, that has already employ'd the Learned.

But, besides those many Beauties in their Forms, which, of themselves, are a sufficient Reason for their Examination, and well worth any Pains that may be taken about them, there are yet more engaging Inducements to excite the Curiosity of Mankind to it, which arise upon the Dissection of the Seeds. Of this I have subjoined a few Examples, which I hope will be entertaining to this learned *Society*, as a *Specimen* of many Discoveries that will follow, in the Course of my Observations on the great Number of Seeds, that shall be the Subjects of my Study for some time.

The first is the Seed of the *Musk Scabious*, which, for its Shape and Structure, is amazing. It resembles an octagonal *Vase* with a scalloped Brim: the Whole is Bell-shaped, having Ribs or Divisions, which run down from the Mouth of the *Vase*, and, becoming narrower, form the Bottom: Between these Ribs, down to the Beginning of the narrow Part, it is clear,

tho' not quite transparent; and, from thence to the Bottom, the Ribs are hairy.

This *Vase* contains a Seed, which is like a *Pestle* standing in a *Mortar*: the *Pestle* is loose in an octagonal Case; but the Narrowness of the Mouth of this Case hinders the *Pestle's* being drawn out, because its Extremity, within, is round and bulky. From its upper End arise five spiculated *Aristæ*, whose little Thorns are directed upwards, and are thereby prepared to cause the Seed to recede from any thing that might injure it upon being touched; and the Basin, from which the *Aristæ* rise, is of a fine green Colour, They are of a shining Brown.

The second *Specimen* is that of the *Angelica*. It is one of the most fragrant and agreeable Seeds, for its Smell, in the World. When the Husk is pull'd off, the *Nucleus* appears of a brownish Colour, and its Shape is elliptical. By the Help of the *Microscope*, we know what produces that charming Smell, being a fine Amber-coloured *Gum*, which appears in Ridges disposed alternately, with others of a brownish Colour, in a longitudinal Direction all over the *Nucleus*. What appears white, on the flat Side, is a *Thoma*, which receives a very minute *Stilus* from the Pedicle that supports it.

The third is that Seed which is vulgarly call'd *Grains of Paradise*. This Seed, altho' promising from its Aspect but very little that is curious, being only a brown irregular Seed with Flats and Angles, and having an *Apex* like the Mouth of a Purse drawn up with a String; yet, when dissected, nothing can produce a more beautiful Appearance. In a longitudinal Section, you see, first, the Edge of the brown
Cortex;

Cortex; next to that, a black pitchy Substance; and, within that, an exceeding white radiated Matter, which looks like a fine white Salt, and is, probably, a Mixture of a volatile pungent Salt with a farinaceous Substance: The Radiation seems to confirm this Opinion; for, if it were only a *Farina*, it could have no such Appearance, and so does its exceeding sharp Taste. But the most remarkable and curious Part of this Seed, is a little Piece of *Camphire*, exactly shaped like a common Vinegar-Crewet, having a round Bottom, and a long taper Neck. This is the constant Form in Hundreds of these Seeds that I have cut. These curious Appearances, I believe, were never observed before.

As I would always endeavour to avoid Prolixity, in any thing I have the Honour to lay before you, I shall, at present, only mention one more Seed, which is that of the great *Maple-Tree*. It consists of a *Pod* and its *Wing*: Two of these grow upon a Foot-stalk with the Pods together, which makes them resemble the Body of an Insect with a Pair of expanded Wings. The Wings are finely vasculated, and the Pod is lined with fine silky Down, which contains a round compact *Pellet* cover'd with a brown Membrane, that sticks very close to it. When this is peel'd off, instead of discovering a Kernel, as in other Seeds, an intire *green Plant* appears to be folded up in a most surprising manner, whose *Pedicle* is about two Eighths of an Inch long, and its seminal Leaves about six Eighths each; between which the *Germina* of the next Pair of Leaves are barely visible to the naked Eye, but plain with a *Microscope*. This Discovery gave me great Pleasure, as believing myself

myself the only one who had observed it ; but, some time after, looking into *Derham's Physico-Theology*, upon another Account, I found it mention'd, as if Dr. *Highmore* had seen and communicated it to Mr. *Ray*. I believe, however, as none of this learned *Society* have seen it, except those I have shewn it to, the Sight of it will not be disagreeable.

Numbers of such amazing *Phænomena* appear every Day in my Observations (some of which shall hereafter be laid before you, if these prove acceptable), which excited me to a Resolution, of examining and describing all the *Genera* of Seeds. A Work which is now publishing under the Title of *The Microscopical Theater of Seeds*, &c. in a manner, which, I hope, will render Botany more easily understood, will hand down to Posterity the true Figures of every Seed and its Sections; and, by the new Discoveries, which often occur thro' the Course of my Observations, lay a Foundation for future Observers to build something useful upon; and settle some Points relating to the different Substances contain'd in Vegetables, which yet remain doubtful.

These were my Views in undertaking this Work ; and from what I have said, I hope it appears, that since those Things, which are capable of being view'd and consider'd by the naked Eye, have been thought worth the Care of describing and delineating, the Objects before us much more want that Care, whose natural Beauties cannot be inspected nor enjoy'd, without Assistances which every one cannot make due Use of.

XII. *A Catalogue of the FIFTY PLANTS from Chelsea-Garden, presented to the ROYAL SOCIETY by the Company of Apothecaries, for the Year 1742. pursuant to the Direction of Sir Hans Sloane, Bart. Med. Reg. & Soc. Reg. nuper Præs. By Joseph Miller, Apothecary, Hort. Chels. Præs. ac Prælector Botan.*

- 1001 **A** Brotanum Lini folio acriori & odorato. *Tourn.* Draco herba. *Park.*
- 1002 Abutilon *Americanum* Ribesii folio, flore carneo, fructu pentagono aspero. *Houft.*
- 1003 Aconitum coeruleum, sive *Napellus*. *C. B.* 183.
- 1004 Aconitum ~~Lycostonium~~ luteum. *Ibid.*
- 1005 Ammi majus *Off.* *C. B.* 159.
- 1006 Anchusa purpurea. *Park.*
- 1007 Anonis non spinosa viscosa hirsuta, odore Theriacæ. *Hort. Cathol.*
- 1008 Arum *Africanum*, flore albo. *Parad. Bat.*
- 1009 Balsamita major. *Dod.* Costus Hortorum. *Off.*
- 1010 Bidens folio tripartito, diviso. *Tourn.*
- 1011 Buglossum latifolium sempervirens. *C. B.*
- 1012 Buglossum orientale, flore luteo. *Tourn.*
- 1013 Campanula hortensis, folio & flore oblongo. *C. B.* 94.
- 1014 Campanula Persicæ folio, flore albo pleno. *Lob.*

- 1015 *Carduus acaulis minor*, flore purpureo. *C. B.*
380.
- 1016 *Carduus aculeatus*, *Ptarmicæ Austriacæ folio Triumfetti.*
- 1017 *Cnicus*, *Atractylis lutea*, dictus, *Off. Hort.*
Lugd. Bat.
- 1018 *Carum Off.*
- 1019 *Doronicum Americanum. Park.*
- 1020 *Elichrysum*, seu *Stoechas citrina angustifolia.*
C. B.
- 1021 *Elichrysum latifolium Americanum. Tourn.*
- 1022 *Erysimum polyceratium vel corniculatum.*
C. B. 101.
- 1023 *Fabago Belgarum*, five *Peplus Parisiensium,*
Lugdunens.
- 1324 *Filix mas aculeata*, pinnulis auriculatis angustioribus. *Raii.*
- 1025 *Genista juncea. J. B. Hispanica. Ger.*
- 1026 *Gentiana Alpina*, magno flore. *J. B.*
- 1027 *Glaucium* flore luteo. *Tourn.*
- 1028 *Gramen Dactylon esculentum. C. B. Manna*
vulg. H. L. Bat.
- 1029 *Helleborus niger fetidus. C. B. 186.*
- 1030 *Hermannia*, folio *Lavendulæ obtuso*, flore parvo
aureo. *Boerhaave.*
- 1031 *Hieracium montanum tomentosum. Morison*
Hort. Reg. Blæs.
- 1032 *Horminum pratense*, flore minimo. *Schol.*
Botan.
- 1033 *Horminum sylvestre*, *Lavendulæ* flore. *C. B.*
239.
- 1034 *Ilex*, oblongo ferrato folio. *Ib. 234.*
- 1035 *Ketmia Syrorum*, flore albo. *Boerhaave.*

- 1036 *Lentiscus vulgaris*. C. B. 399.
 1037 *Lingua cervina multifida*. *Ibid.* 354.
 1031 *Lotus hæmorrhoidalis major*. *Park.*
 1039 *Lupinus sativus*, flore albo. C. B. 347.
 1040 *Lupinus sylvestris*, flore luteo. *Ibid.*
 1041 *Lupinus peregrinus major villosus cæruleus*. *Ibid.*
 1042 *Lychnis Chalcedonica*, flore miniato. *Park.*
 1043 *Melilotus major candida* *Tragi.*
 1044 *Melilotus odorata violacea*. *Hist. Oxon.* *Lotus*
 urbana. *Off.*
 1045 *Moldavica Americana trifolia*, odore gravi.
 Tourn.
 1046 *Origanum*. *Off.* *Origanum Anglicum*. *Ger.*
 1047 *Ochrus folio integro capreolas emittente*. C. B.
 243.
 1048 *Orobis vulgaris herbariorum*. *Ger. Park.*
 1049 *Panicum Indicum* spica longissima. C. B. 343.
 1050 *Punica malus*. *Off.* *Malus Granata*. *Ger.*

XIII. *A Letter from the Rev. Mr. Roger
 Pickering, F.R.S. to the President; concerning
 the Manuring of Land with fossil Shells.*

S I R, Charles-Square, Hoxton, Nov. 22. 1744.

Read Dec. 6. 1744. **I** TAKE the Liberty of offering, by your
 Hands, to the Society, a Specimen of
 fossil Shells, lately sent me, which are pretty per-
 fect; and, on account of the Place from whence
 they were taken, remarkable. At *Woodbridge* in
Suffolk, in a Farmer's Ground, there are some Pits,
 in Depth equal to the usual Height of Houses, con-
 B b sisting

sisting of several *Strata* of Shells from the Bottom to within about nine Feet of the Surface, where the natural Soil of Gravel and Sand begins. The Mass of Shells here collected is prodigious; the Sorts various; but that Kind which I have taken the Liberty to produce, and which, I apprehend, is the *Buccinum vulgare*, or Whilk, prevails the most. The Shells before you were taken up from the Bottom of the Pit, where the Depth to which these Shells reach is not yet dug down to. *Woodbridge* is seated seven Miles N. E. from *Ipswich*; and is about the same Distance from *Orford* on the Sea coast, which bears from it due East. How, therefore, such a Mass of Shells should get there at such a Distance from the Sea, when History has inform'd us of no remarkable Inundation in those Parts, or that such a Tract of Land was ever recovered from the Sea, appears to me difficult to determine, by any other than the *Mosaic* Hypothesis of an universal Deluge. 'Tis true, indeed, the River *Deben*, which rises at *Debenham* some Miles off, runs by *Woodbrige*, within half a Mile of these Pits, in its Course to the *German* Ocean, where it empties itself: But such a Collection of Shells can hardly be supposed to have been thrown up by it, and a Surface of Earth, to the Depth of nine Feet, settled over it, without allowing a Space of Time for such a Circumstance, almost equal to the Interval between us and the Deluge. But, however these Things be, the Farmer, in whose Ground these Shells are, has, as I am informed, laid the Foundation of an ample Fortune from them. The Man contented himself in the old beaten Track of the Farmers (a Behaviour which does infinite Prejudice to the Improvement of Natural Knowledge.

ledge in Agriculture), till an happy Accident forced him upon a bold Improvement. He used to mend his Cartways, when broken up by Harvest-Work, with these Shells; in which Business his Cart one Day broke down, and threw the Shells out of the Cart-Track into the cultivated Part of the Field. This Spot produced so remarkable a Crop next Year, that he put some Loads upon a particular Piece, kept the Secret to himself, and waited for the Event. This Trial answering Expectation, he directly took a Lease of a large Quantity of poor Land, at about five Shillings the Acre; and having manur'd it heartily with these Shells, in about three Years it turned to so good an Account, that he had 15 Shillings the Acre proffer'd to take the Lease out of his Hands. I know that Manuring Land with Shells; those of Oysters in particular, is no Novelty: I mention this with Regret, as an Instance of what poor Hands, both as to Landlords as well as Tenants, Agriculture, an extensive Branch of Natural Knowledge, is generally thrown into; which both requires and deserves the close Attention of a philosophical Mind *.

It is with true Respect and Esteem, that I have the Honour, Sir, of being,

Your most humble Servant,

R. Pickering.

* It might be of great Service to the Public, if every curious Gentleman, who holds Lands in his own Hands, would allot an Acre, or half an Acre only, for making Experiments; would carefully set down his Observations, and then send them in to the *Royal Society*; there to be recorded, or published. C. M.

XIV. *Abstract of a Letter from Mr. Wm. Arderon to Mr. Baker, F.R.S. of a Shuttle-Spire taken out of the Bladder of a Boy.*

Norwich, Oct. 12. 1744.

Read Dec. 6. 1744. **O**N the 16th of September last, Mr. John Harmer, a Surgeon in this City, cut one Peter Riggs, a Boy about seventeen Years of Age, for the Stone; at which time there was extracted from him (to the utmost Surprise of the Spectators) an iron *Shuttle Spire*, four Inches long. He had, it is said, some time before, a Stoppage of Urine; and, by endeavouring with this Piece of Wire to relieve himself, and thrusting it too far along the urinary Passage, he let it drop into the Bladder, where it occasioned the same Symptoms as a Stone would have done. He underwent the Operation with great Fortitude; and said nothing of this Accident until it was all over. He is now perfectly recovered.

XV. *An Account of a remarkable Cure, performed on the Eye of a young Woman in Scotland, by Tho. Hope, M. D. communicated from Dr. Mead.*

Read Dec. 13. 1744. **J**AANE Willson, a Girl now eighteen Years of Age, about seven Years ago began to have her left Eye turned towards the Temple, occasioned by some Tumour betwixt the Globe and the Orbit. This Tumour, for some Years, did

.VIX

not

not appear outwardly ; but, increasing by degrees, at last a hard Swelling appeared externally, reaching from the great Angle almost to the little Angle under the lower Eyelid, and half an Inch down on the Cheek : It had forced the Globe of the Eye almost out of the Socket, so that the *Pupil* of that Eye was, by Measure, above three Quarters of an Inch further from the Nose, than the *Pupil* of the other Eye ; and the Eye was more jetting out in proportion ; so that it seem'd to be out upon the Temple, and quite immoveable ; which, with the Tumour, made a frightful Sight. The Patient had frequent Pains in her Head ; but what was most surprising, the Sight of that Eye was not lost, tho' a good deal impaired.

I shew'd this Patient to Mr. *Alexander Monro*, Professor of Anatomy at *Edinburgh*, whose Abilities are universally known ; who, after examining it very narrowly, gave it as his Opinion, that this Tumour had begun at the Bottom of the Orbit ; and that the Extirpation would be exceedingly difficult ; and, as it seem'd to be an incysted Tumour, if any of that *Cystis* remain'd at the Roots, it would be apt to sprout up again : But, withal, concluded, that there was Room for a Trial ; and it would be a Pity not to do something in order to save the Patient's Eye, and, 'probably, her Life, which would be in Danger, if the Tumour continued to increase. I likewise shewed her to several other eminent Gentlemen of the Faculty, who were all pretty much of the same Opinion.

Notwithstanding of this, considering the great Risque that the Patient run, if something was not speedily

speedily done, I resolved to undertake it ; having had a Case of the like Nature, but in a lesser Degree, under my Care about twelve Years ago in *London*, the Extirpation of which I performed without any bad Consequence ; and, upon consulting my old Master *St. Yves* his Book, I found almost a parallel Case to this Girl's, which, he says, he extirpated with Success ; and, as he was an honest Man, I knew I could trust to him.

On the 19th of *June* last, in Presence of Dr. *Lewis*, Dr. *Dundas*, Dr. *Mac-Farlane*, Dr. *Young*, Mr. *Cunningham*, Surgeon, I performed the Operation in the following Manner :

I turned the Patient backwards on a Chair upon an Assistant's Lap, her Head supported by Pillows ; then, keeping the Skin tense with my Fingers, I made an Incision about an Inch long with a small Razor, beginning at the greater Angle, and following the Direction of the Fibres of the orbicular Muscle towards the lesser Angle. I then passed a crooked Needle armed with Silk thro' the Middle of the Tumour as deep as I could go ; and, raising the Tumour with the Silk, with a fine Bistoury I separated all the lateral Adhesions from round the Tumour ; and, with the Point of my Scissars, I cut the deeper Adhesions, which I could not so well reach with the Bistoury, and brought away all that the Thread had hold of. This seemed to be a tough membranous Substance, independent of the real Tumour ; for, after this was quite taken out, there appeared a regular Tumour, of a spherical Figure, smooth and even, about the Bigness of a small Pigeon's Egg : I passed the Needle thro' the Middle of it,

as I had done before, and plunged a Lancet into it as deep as I could, in order to let out any fluid Matter that might be contained therein, but found nothing but a carnous Substance; then, lifting up the Tumour by the Thread, I dissected it, with great Care and Caution, from the adjacent Parts, as far as I could; in doing this, I found several strong callous Attachments on the Side next to the Globe, which felt almost as hard as a Cartilage, and obliged me to change two or three Instruments. I then, with the Point of my Scissars, cut the inward Adhesions at the Roots, and brought the Tumour away intire: Upon putting in my Finger to the Bottom of the Orbit, I could feel several hard callous Substances stil remaining; and keeping my Finger upon them, I slid a crooked Needle armed with Silk round the Point of my Finger, with which I hooked those callous Roots; then, making an Assistant raise the Thread, and directing the Scissars upon the Point of my Finger, where I felt the said Roots, with two or three Snips I cut them quite away; so that I left the Bottom even, and intirely free, as far as I could judge. All this while I had no great Effusion from any Artery, but a good deal of black grumous Blood from the varicose Vessels, I dressed it up the first time with dry Lint, which I did not take off till the third Day; when I found a soft Swelling in the Eye-lids and *Conjunctiva*, with a slight Inflammation, and a Pain in the Forehead. I dressed the Wound with a soft Dossel dipt in common digestive and warm Brandy, and ordered an emollient Fomentation to be applied every two Hours: The Pain in the Forehead, and the Swelling continued for three or four Days, with-

out

out any Appearance of Matter. I then touched the Bottom of the Wound with the lunar Caustic, and some Hours after, there followed a pretty large Discharge of blackish Blood, and immediately her Head was relieved, and the Swelling subsided : A bloody *Sanies* continued to issue out the two following Days, for which I injected warm Water, with a little Brandy and Honey of Roses, after which it came to a pretty good Digestion : As some spongy soft Substances began to appear, I touched them with the lunar Caustic, and the Wound filled up apace. The Eye still continued immoveable, the *Musculi Abductores* had been so long contracted, and the *Abductores* so over-stretched and lengthen'd, that they had lost their Use; I could however observe, that, by pressing with my Hand upon the Globe of the Eye with a little Force, I could bring it a good deal more into the Socket, but, upon taking away my Hand, it would immediately return to its former Place. This made me think, that a constant and gradual Pressure, by some proper Bandage, might be of Service to force the Globe into its Place, and keep it there till the Muscles had recover'd their Tone : Accordingly, I got a steel Bandage, with a concave brass Plate corresponding to the Convexity of the Eye; which, by the means of a Screw, bore upon the Side of the Globe next the Temple: I applied this Bandage, by first gently forcing the Globe more into its Place with my Hand; then, putting a thick soft Compress betwixt the Globe and the brass Plate, I screwed it down upon the Globe in such a manner, that it was impossible for it to start back again as it used to do, I left an Assistant with the Patient all Night, with Instructions, If the Bandage caused great Pain, to ease the Screw;

Screw; and so, by gradually forcing it more and more, and keeping this Bandage constantly applied Day and Night, in about twenty Days the Eye was brought intirely into its Place, so as to remain there of itself, had all its regular Motions every Way, and the Patient saw with that Eye as well as with the other.

This Patient, after the Cure, was shewn to the Physicians that had been present at the Operation, and to others the most eminent of the Faculty. In the Morning, when I used to take off the Bandage, I could observe that Side of the Globe which the Plate bore upon considerably flatten'd, and yet not attended with any Pain, or bad Consequence. In about a Month the Wound was quite healed up. A spongy Carnosity had grown all along the Inside of the lower Eyelid, which, being long over stretched by the Tumour, was so relaxed, that, after the Operation, it turned inside-out, and occasioned that Disorder which is called *Ectropion*: The upper Eyelid having been very much extended for so many Years by the Globe, upon the Eye returning to its Place, was so relaxed, that its Cartilage, on the contrary, turned inwards; whereby the *Cilia* or Hairs upon its Borders rubb'd against the Globe of the Eye, and occasion'd the Disease commonly call'd *Trichiasis*. For the Cure of the *Ectropion*, I pass'd a crooked Needle thro' the Middle of the Carnosity, and raising it by the Thread, I cut it off with the Scissars; I afterwards touch'd the Inside of the Eye-lid with the lunar Caustic, in order to destroy what remained of the Carnosity; and, giving the Eschar Time to throw off, I repeated the same twice or thrice, by which the

G c.

Eyelid,

Eyelid, in about a Fortnight, recovered its healthful and proper Situation. I did not think proper to torment the poor Patient with the Operation of the *Trichiasis*; which, tho' very easy to the Operator, is not so to the Patient; and I found, by the Application of proper Topicks, the Eyelid recovered Strength daily; and I judged by the Continuance of the same Method it would soon be well.

It will not be easy to account how Sight should remain after the Optic Nerve was so stretched; which it must be in a very considerable manner in this Case; and how it came to contract or recover itself so soon, after being so great a while extended. It is true, that while the Optic Nerve was in its State of Extension, the Sight was impaired; but, after seven Years Extension, how it came to recover itself in a Month's time, without any Alteration in the Sight, but for the better, I leave to the Speculation of the Curious. It is likewise pretty extraordinary how the Muscles, after having been so many Years in Disuse, should recover their natural Action in so short a time. *See TAB. II. Fig. 8.*

XV. An Explication of a Roman Inscription found not long since on a Stone at Silchester in Hamshire: By John Ward, Rhet. Prof. Gresh. and F. R. S.

Read Dec. 13.
1744.

THE Draught, which accompanies this Paper, contains an exact Copy of a *Roman* Inscription, lately communicated to me by Dr. *John Collet*, Physician at *Newbury*. The
Account,

Account, which he gives of it, is this : The original Stone, in which it is cut, was found at *Silchester*, within the antient Market Place, about four Feet under ground; and is now in the Possession of Mr. *John Stair* of *Aldermarston*, who took this Copy of it by pressing the Paper into each Letter and Mark in the Stone, so that every Part is exactly of the same Size and Form with the Original. The two Stops in the Shape of Leaves are not cut so deep as the Letters, or the triangular Points. The Stone is one Inch and five Eighths in Thickness; and those Parts of it, which are wanting both at the End of the Lines, and at the Bottom, were broken off and lost, before it was dug out of the Ground.

With that larger Draught I have also sent a lesser, by a Scale of one Fourth of it (*see* TAB. II. *Fig.* 9.). And from the usual Form of such votive Inscriptions, and the Manner of expressing them, I apprehend, there are not many Letters lost at the End of the Lines, and but one Line wanting at the Bottom; so that if all the Words were written at Length, and the Line, which is wanting, supplied, the Whole would run in the following Manner.

*Deo Herculi Segontiacorum Titus Tammonius,
Saenius Tammonius Vitalis, cornicularius, ho-
noris causa dedicarunt vel fieri curarunt.*

We find no less than six Altars dedicated to *Hercules* in Mr. *Horsley's Britannia Romana*, two of which have the Title DEO prefixed to the Name HERCVLI (a), as in this Inscription. But as the

C c 2

Thinness

(a) Northumb. num. LXXXI, xciv.

Thinness of the Stone shews, that it could not be Part of an Altar, it might probably belong to some public Building erected to his Honour at this Place. Mr. *Stair*, as Dr. *Collet* informs me, has drawn a Plan of the antient Town, the Traces of which are sometimes visible in Summer; as likewise the Ruins of an Amphitheatre, without the Wall, not far from the Eastern Gate. This would have led me to imagine, that this Stone might formerly have been removed from thence; but that I cannot meet with any Instance of an Amphitheatre dedicated to *Hercules*, tho many Temples and other Buildings were erected in Honour of him, as may be seen in *Gruter*.

The Word SEGON. in the second Line, must, I think, be read SEGONTIACORUM, as referring to the Name of the People *Segontiaci*. Thus we have in Mr. *Horsley* DEAE NYMPHAE BRIGANTVM (a), and MOGVNTI CADENORVM (b); and in Mr. *Camden* DEO MOVNO CADENORVM (c); denoting the topical Deities of those People. For as to the Town *Segontium*, notwithstanding the Affinity of its Name with the *Segontiaci*, it was at a great Distance from them, as appears by *Antonine's Itinerary* (d), being situated on the Western Coast over against the Isle of *Anglesea*, where *Caernarven* now stands; and therefore it could have no Relation to this Inscription.

The three following Lines contain the Names of the two Persons, who caused this Dedication to be made

(a) Pag. 269, 315.
p. 663, edit. 1607.

(b) Northumb. num. LXXX.
(d) Iter xi.

(c) Brit.

made in Honour of *Hercules*, that is, TITVS and SAENIVS TAMMONIVS, that of VITALIS in the fifth Line being a *Cognomen* of the latter, which often occurs as such in *Gruter*.

The two imperfect Letters at the Beginning of the sixth Line I take for OR, which with C before them, now broken off, making the Syllable COR might stand for an Abbreviation of the Word CORNICVLARIVS. We find CORNICVL. for CORNICVLARIVS both in Mr. *Horsley* (a) and *Gruter* (b); and *Reinesius* reads K. LG. XIII. *cornicularius legionis decimæ tertie* (c), K being put for C, which was not uncommon. And I believe it would not be easy to find any other Word, that would suit the Reading in this Place. *Cornicularius* was a general Title for a Clerk or Secretary to a military Corps; or some superior Officer, military or civil. Hence we meet with *cornicularius cohortis, legionis, tribuni, præfecti, consulis*, as also *tribuni plebis* (d). And in the *Notitia dignitatum imperii Romani*, published by *Labbe*, this is one of the Officers said to belong to the Court of the *comitis littoris Saxonici per Britanniam* (e), who commanded this Part of the Island. *Saenius Tammonius* therefore might possibly be that Officer, and choose in this Manner to join with the other *Tammonius* in shewing a Regard to the tutelary Deity of the Country, where they resided.

As there can be no Doubt, but the other Word in the same Line, when perfect, was HONORIS; who-

(a) Northumb. LXIV. 6. (b) Pag. DXLV. num. I. (c) Class. I. num. 192. (d) See Pancirol. Comment. in Notit. Dignitat. p. 11. (e) Sect. LII.

whoever considers the common Forms of such votive Inscriptions, will readily supply the Word CAUSA with DD. or F.C. for DEDICARVNT or FIERI CVRARVNT in a following Line, now broken off, as necessary to complete the Whole.

Had this Inscription been sooner discovered, it would have saved our Antiquaries much Trouble in fixing the Situation and Limits of the *Segontiaci*; about which they have been greatly at a Loss, and led into different Opinions. Those People are first mentioned by *Cæsar*; who in the Account of his second Expedition into *Britain* says, that the *Trinobantes* having submitted to him, the *Cenimagi*, *Segontiaci*, *Ancalites*, *Bibroci*, and *Cassi*, followed their Example (a). The *Trinobantes* are placed by Mr. *Camden* in *Middlesex* and *Essex* (b), and the rest in the neighbouring Counties on each Side the *Thames*; the *Segontiaci* particularly in the North Part of *Hampshire*, in *Holeshot Hundred* (c). And he rightly takes *Vindonum* or *Vindomis*, as it is called by *Antonine* (d), now *Silchester*, to have been their principal Town. But tho Dr. *Gale* agrees with *Camden* in making *Silchester* the same as the antient *Vindomis*; yet he thinks, that Town did not belong to the *Segontiaci*. His Words are these: *Segontiaci oram maritimam circa Cicestriam, olim Caer Cei, longe infra hanc urbem tenuerunt; & errant, qui credunt Vindonim eprum fuisse civitatem* (e). Mr. *Horsley* differs from them both; and neither admits *Silchester* to be the antient

(a) B. G. Lib. v. c. 20, 21.

(b) Pag. 298. edit. 1607.

(c) Ibid. p. 194.

(d) Iter XII. xv.

(e) Comment. in

Antonin. Iter. Brit. D. 125.

antient *Vindomis*, nor to lie within the Bounds of the *Segontiaci*; but takes it for *Calleva Atrebatum* (a), mentioned likewise in the *Itinerary* (b). From the Difficulty therefore of fixing the Situation of the *Segontiaci*, Dr. *Glarke* contents himself with only placing the Word *incertum* against their Name (c). But had this short Inscription, as imperfect as it is, offered itself to these learned Writers, none of them could have been at any further Doubt, either in placing *Vindomis*, and not *Calleva* (which belonged to the *Atrebatas*) where *Silchester* now stands; or including this Town within the Limits of the *Segontiaci*. The Want whereof has likewise occasioned them no less to differ in settling some other neighbouring Stations, which by this Help might have been fixed with much more Agreement and Certainty.

I would beg Leave further to observe, with regard to the Persons concerned in this Dedication, that Mr. *Camden* has published the following Inscription found long ago at *Silchester*, MEMORIAE FL. VICTORINAE T. TAM. VICTOR CONIVX POSVIT (d); wherein the abbreviated Names T. TAM. are read by Mr. *Horsley* TITVS TAMPHILVS (e); the latter of which occurs indeed in the *Fasti Consulares*, but as a *Cognomen*, M. BAEBIVS TAMPHILVS. Wherefore I am more inclined to think, it should be read TAMMONIVS, as it has there the Place of a Family Name; and that probably he was the

(a) Brit. Rom. p. 457.
Propior. nom. apud Caes.
(e) Brit. Rom. p. 332.

(b) It. VII. XII. (c) Ind.
(d) Britann. p. 196. edit. 1607.

the TITVS TAMMONIVS mentioned in this other Inscription. It is true, that he has the *Cognomen* VICTOR given him in the former, which does not appear in this; but either he might not have gotten that till afterwards, or the initial Letter V might have stood at the End of the third Line after TAMMONIVS, which, as several Writers observe, was antiently put for VICTOR. And it is very remarkable, that *Gruter* has given us a like votive Inscription, erected by two Persons, having both the same Family Name, with the *Cognomen* VICTOR added to one, and VITALIS to the other, like those in the Instance before us. *Gruter's* Inscription runs thus: APOLLINI. SACRVM. EX. VOTO. C. VIRIVS. VICTOR. ET. L. VIRIVS. VITALIS. S. L. M (a). It is not improbable therefore, that in both Cases the two Persons were either Brothers, or nearly related to each other.

(a) Pag. XXXVIII, num. 17.

Gresham College,
December 8,
1744.

John Ward.

XVII. *A Letter from John Huxham, M. D. F. R. S. to Cromwell Mortimer, M. D. Secr. R. S. serving to accompany an Account of the Case of one Hannah Hitchcock, one of whose Ureters was grown up; a Present of a beautiful Stalactites, now in the Museum of the Royal Society; and a Drawing of an extraordinary Calculus taken out of the Bladder of a Boy.*

Dear Sir,

Read Dec. 13. 1744. **I** HAVE now an Opportunity of sending you, inclosed, the Case of one *Hannah Hitchcock*; and a little Box, containing the Stones taken out of her Kidneys.

In the Box also I have sent one of the most remarkable *Stalactites* I ever saw. Perhaps it may be a Curiosity even to *you*. — It was found in a Cavern, that was discovered amidst the vast Marble Rocks at *Cat-down* near *Plymouth*. — It hung perpendicularly from the Top of the rocky Cavern, and was a cylindrical Tube of twenty Inches long at least; but was unluckily broke into several Pieces in Bringing to me. This I have sent was by much the longest of them; but Mr. *Long* (the Master of the Quarries) assured me the Whole was above twenty Inches long, and quite cylindrical, and quite hollow. — I went to the Cave the next Day, and found five or six of such Kind of Tubes, but none above two Inches long. — They all sprang from a broad, hollow, protuberating Basis, in some sort as a Nipple arises from the Breast. —

D d

These

These also were cylindrical and hollow. — There were in the same Cavern many other Petrifications, which had formed a kind of hollow Pilasters against its Sides; and also several large solid Masses, which arose from the continual Dropping of the petrefying Water through the Crevices of the superior Rock. — These all afford very good Alabaster.

In the Box likewise you will find an exact Draught of a Stone (*see* TAB. II. *Fig.* 10.) lately taken out of the Bladder of a Boy about twelve Years old; which I think of a pretty uncommon Figure; tho' not indeed so very remarkable as that mentioned *Philosoph. Transf.* N^o 450. — The Boy died two or three Days after the Operation: So that the Parents keep the Stone as a Relique of their unfortunate Child, and will not part with it. Dear Sir, I have the Honour to be

Your much obliged, and

Plymouth, Sept. 20.
1744.

obedient humble Servant,

J. Huxham.

The CASE of *Hannah Hitchcock*.

H*annah Hitchcock*, about Sixty, was from her Youth up a very sober industrious Woman, of a thin Habit of Body, and bilious Constitution: But in her latter Years she became unhealthy, and was frequently subject to Fits of the Gravel, and brought off some small Stones.

For about three or four Years before her Death, she was often afflicted with nephritic Colics, great Pains

Pains of the Stomach and Back, Suppression of Urine, and sometimes discharg'd bloody Urine. — She at length became ascritical and asthmatic; and, for more than eight or ten Weeks before she died, she had a violent Pain in the Region of the Stomach, and a hard Swelling under the *Cartilago ensiformis*, with almost perpetual Vomitings, colical Pains, extreme Costiveness, and Difficulty of Urine. The last Fortnight, or more, she vomited every thing, and had a total Suppression of Urine for fifteen Days. — At last, she died comatose and convuls'd.

Upon opening the *Abdomen*, soon after her Death, there neither appeared Stomach, Liver, or Guts, but a large irregular Mass, spread from one Side of the *Abdomen* to the other, and adhering firmly to both Sides. — This was, in great part, the *Omentum*, grown, as it were, cartilaginous, and as tough almost as Leather; having up and down in it, several large, very hard, scirrhus Nodes, and some Tubercles, full of fetid *Pus*.

The Stomach was very much contracted, but its Coats were very thick; and, near the *Pylorus*, very much inflamed, almost mortified; its Glands, in several Places, scirrhus, and as large as Peas. — The Passage from the Stomach to the Guts was shut up, partly by the Inflammation and Thickness of its Coats, and partly by its odd Coalition with the Liver and *Omentum*.

The Liver was much shrunk and scirrhus, and roll'd up into a kind of conical Figure; in the *Vertex* of which appeared the Gall-bladder, of a dark-green Colour, and very turgid; and yet the *Ductus*

communis Cholodockus was near four times as large as usual.

The *Ilium* was thrust down much lower than ordinary; and the Convolutions of the Gut were, in several Places, grown firmly together: The *Colon* also, on both Sides, was strongly attach'd to the *Peritoneum*. 'Tis almost constantly observed, where the *Omentum* is either consumed, or greatly depraved, that the Convolutions of the Guts grow together, and adhere to the adjoining Parts, for want of that oily *Mucus*, which, in a natural State, in great Plenty transudes from the *Omentum*, to lubricate them, and render them fit for the regular Performance of the peristaltic Motion, &c.

In the Cavity of the *Abdomen* there were near six Quarts of putrid Water, somewhat ting'd with Blood — in the right Cavity of the *Thorax* about a Pint.

The urinary Bladder, quite empty, and half rotten, did not contain a Drop of Urine, but was smeared over with a sort of purulent Matter. In the right Kidney was found the bigger Stone, which took up almost all the *Pelvis renalis*. — In the right Ureter were two or three small Stones, which, with a sort of very tough *Mucus*, had shut its Passage intirely. — Indeed the great Stone in the *Pelvis* had well-nigh quite bung'd up the Mouth of the Ureter.

In the left Kidney was the smaller Stone, which had so entered the Ureter with its conical Part, as adequately to stop it, like a Cork in a Bottle. When we had removed this Stone (tho' we found no other in the Ureter), we could not force any Water thro'
it

it into the Bladder, altho' a strong injecting Syringe was used ; for the Ureter was greatly contracted, and there seem'd an absolute Coalescence of its Sides.

'Tis certain, the Canals of a human Body, that cease to have any Fluid transmitted thro' them, soon coalesce; as is particularly evident in the *Canalis arteriosus, Urachus*, umbilical Vessels:—Nay we see the external Coats of the Intestines soon grow together, if they are not constantly lubricated with their proper oily *Mucus* that prevents it. This will be much sooner effected in the Ureters, if they happen to be excoriated by small Stones, or Gravel: For we find even the Fingers, or other excoriated Parts, soon grow together, unless prevented by due Care.—I am persuaded this is sometimes the Case in fatal Ischuries; tho' more frequently they arise from obstructing Gravel, Stones, grumous Blood, tough Mucosity, or the like.

XVIII. *A Letter from Edward Wilmot, M. D. F. R. S. and one of His Majesty's Physicians, &c. to the President of the Royal Society, serving to inclose the two following Papers :*

1. *Of the extraordinary Effects of Musk in Convulsive Disorders; by J. Wall, M.D.*
2. *Of the Effects of the Tunquinese Medicine; by Alex. Reid, Esquire,*

S I R,

Read Dec. 20.
1744.

Inclosed you have some Observations upon the Virtues of Musk, from Dr. Wall, an eminent Physician at Worcester. I have likewise sent you an Account, I have procured from my ingenious Friend Mr. Reid, of the Event of some Experiments made by him with the same Drug; which, being communicated to his Friends, gave Occasion, probably, to the liberal Use of Musk, of late Years in Practice here. As both these Accounts well merit the Attention of the Public, I could wish they might be both printed in the same Transaction. I am, with great Respect,

S I R,

Jermyn-Street,
Nov. 22,
1744.

Your most obedient,
most humble Servant,

E. Wilmot.

I.

Of the extraordinary Effects of Musk in Convulsive Disorders ; by J. Wall, M.D.

IT seems highly probable, that the Virtues of several Articles in the *Materia Medica* are not hitherto fully discovered, or well ascertained, from Want of due Attention to their proper Doses. The Success of some Medicines, we know, depends upon This; *viz.* That a determinate Quantity be given within a certain Space of Time; so that if Less than that be used, it seldom proves of Use. Of this the *Bark* is a well known Instance; and the ingenious Dr. *Hales* has observed the same of Soap (*a*). It is not improbable, that the like might be found to hold good in many other Medicines, were their Operations carefully enough attended to: A Discovery this, much to be wish'd for, as it would probably furnish us with a new Set of Specifics; and thereby enable us to cure several Distempers, which at present, baffle our Skill. Of the Reasonableness of which Conjecture the Subject of this Paper seems no contemptible Instance.

Musk, and other Perfumes of the same Tribe, have been long celebrated as excellent Antispasmodics; but, as they are usually ordered in very small Quantities, they are seldom found to answer the Expectations of the Prescriber. The Mistake seems to have taken its Rise from hence; That most Persons imagining

(a) Account of some Experiments on Mrs. *Stephens's* Medicines.

gining the medicinal Virtues of these Drugs to consist in their Fragrancy ; and finding that a very small Quantity in Substance would communicate its Smell to a very large Vehicle ; in their Prescriptions therefore they have usually order'd so much only as was requisite to give the whole Composition an agreeable Perfume. But the Case is widely different ; for as a very small Quantity of Musk, Civer, &c. when properly open'd and diluted, will yield a much greater, as well as a more agreeable Fragrancy, than a large Parcel without such Preparation : So the Smell of the Perfume is often found to be of Disservice, where the Substance, inwardly taken, produces the happiest Effects ; as *Etmuller* and others have observed.

Most of the Writers on Pharmacy seem extremely cautious about the Use of Musk ; and therefore direct it only in exceeding small Quantities. In very few of their Compositions does the Dose of it reach so high as a Grain ; and the highest that I meet with is not above five Grains (a). But the *Chinese*, who are much better acquainted with the Nature and Uses
of

(a) In *Fuller's Fulap. Moschat. & Mistur. Moschat.* the Quantity for a Dose is not half a Grain : In the *Pill. Solenandri & Syncopales* only two Grains. *Bates*, in all the Compositions where Musk enters, orders it only in the Quantity of half a Grain to a Dose ; excepting only in the *Fulap. Hysteric. Moschat.* He gives it to the Quantity of five Grains. In this Quantity *Etmuller* once orders it in the like Case. In the College, *Augustan*, and other Dispensatories, the Quantity of Musk in the great Compositions, such as the *Conf. Alkermes Elecl. de Sassafras ; Conf. de Hyacinth. Conf. liberans ; Spec. latifolians ; Diarrhodon Abbat. ; Diamb. c. od. &c.* is very trifling ; in very

of this excellent Drug, order it in much larger Quantities; the usual Dose amongst them being the tenth Part of an Ounce (*b*). In the Powder mention'd by Dr. James, at the End of his Treatise on the Cure of the Bite of a mad Dog, which is said to have been brought from *China*, the Quantity of Musk in each Dose is sixteen Grains, and two of these are order'd to be taken at two Hours Distance.

The Prescription last mentioned had, to my own Knowledge, been given to several Persons with great Success: Two in particular; the one an Acquaintance of mine, a Student at *Oxford*; the other (*c*) a Servant-Maid to Mr. Rogers, an Innkeeper in this City.

In

very few of them amounting to half a Grain in a Dose. *Schroder* makes the highest Dose of Musk four or five Grains. *Lemery* but four Grains. *Hermannus* gives five Grains; but his Commentator *Boecler* gives great Caution about its Use: '*Circa Moschi Usus* (says he) *caute procedere debemus; præsertim in Mulieribus. Sanguinem impetuosum admodum reddit, & Hæmorrhagias Narium facile excitat.*'—And *Juncker*, *Consp. Therapiæ general.* TAB. 18. seems not a jot less fearful of it. For, after he has told us, that it is wonderfully extoll'd in nervous Disorders, and many others, he adds — '*Sed notandum est internum Moschi Usus, nisi parcissimus sit, sæpe noxias Humorū Commotiones inferre; cæterum non adeo probatum, nedum tam gravibus plerisque Malis superandis parem reperiri,*' &c. &c.

(*b*) Vide Du Halde on the Physic of the *Chinese*.

(*c*) This Girl had been bitten about a Year before the Symptoms came on; and had, immediately after the Bite, taken the usual Precautions of being dipp'd in salt Water, &c. She continued well for the Interval; but, about twelve Months after the Accident, she began to be dull, and to have an extreme Dread of the Consequences of the Bite. The Part where she was bitten (which was the lower Part of the Cheek) began to give her great Pain, attended with frequent convulsive Catching therein: At last the Wound broke out, and glored a thin Ichor, with all the Symptoms mentioned above.

In both these the Symptoms of the Bite were come on; *viz.* an ichorous Gleet from the Wound, with extreme Pain in the Part; frequent Convulsions, and *Subsultus Tendinum*; extreme Anxiety, Horror, and Want of Sleep. After the second Dose of the Powders, each of them fell into a gentle Sleep, and broke out into an universal breathing Sweat, both which continued for near eight-and-forty Hours (except only whilst they were taking some Refreshment); after which time they awoke perfectly well; their Sores being healed, and they chearful, and free from all Complaints. In all the Persons (which are several) to whom I gave these Powders, I constantly found them to have the same Effects; procuring Ease, quiet Sleep, and a copious *Diaphoresis*.

These surprising and sudden Effects I could not but attribute, in a great measure, to the Musk (because we well know, that the Cinnabar alone never produces the like): And, from the Safety and great Success with which this large Quantity had been taken, I saw little Reason to be so scrupulous or fearful about its Use, as most Authors appear to have been. On the contrary, I was rather induced to think, that it had hitherto been usually under-dosed; and that very great Advantages might be expected from it, if given in an increased Quantity. Nor was it long before I had an Opportunity of trying it in another Case, where the Success justified my Conjectures, and more than answered my utmost Expectations.

Being called to a Patient at *Bewdley*, a Market-Town in this County, I was desired by Mr. *Malpas*, a very ingenious Apothecary of that Place, to visit a poor Man there, who had lain some time ill. I found

I found him afflicted with a terrible Hiccup, which had continued on him four or five Days without a Minute's Intermiffion; fo that he had had no Sleep all this time. His Pulse was exceedidg quick, fmall, and unequal: The Blood, which had been feveral times, and in large Quantities, taken from him before I faw him, was cover'd with a prodigioufly thick buff Coat; his Urine limpid; his Face of a leaden Hue, and ghafly: his Extremities cold, and cover'd with a clammy Sweat; and whenever the Hiccup return'd, which it did at almoft every Infpiration, the *Hypochondria* were ftrongly convuls'd and drawn upwards. — Recollecting that *Fuller* recommends his *Julap. Moschat.* in the like Cafes (d), but thinking the Quantity of Musk, therein prefcribed, too fmall to be depended on in one fo desperate (more efpecially having before feen the furprifing Succels of a large Dofe), I order'd him a Bolus compofed of Musk, Nitre, Crabs-eyes, of each twelve Grains, Camphire one Grain, Mithridate a fufficient Quantity; to be taken immediately, and repeated in eight Hours, taking a Spoonful or two of *Fuller's Julap*, occasionally, once in two or three Hours. He had no fooner fwallowed the Bolus, which I ftaid to fee him take, than his
Hiccup

(d) In Febribus mali Moris, quæ Vapores deleterios ad Stomachum suffundunt; Spiritusque labantes irritant; in Confusiones & Spasmos adigunt; & Singultum inde excitant; nondum hactenus præstantius novi Remedium: aliquando enim divinitus succurrit, & Singultum superat. Ut ingenue tamen fatear, tam funestum hoc sæpe notavi esse Symptoma, ut neque hoc neque aliud quodcunque vel excogitare vel invenire, mihi adhuc datum fit, quod illi par effet; sed totis licet Viribus tentavi, Fatidis tamen frustra obvisus sum.

Fuller. Pharmacop. extemporan. sub Tit. Julap Moschat.

Hiccup stopp'd: In less than an Hour, he broke out into a general breathing Sweat, and fell into a quiet Sleep, which lasted six or seven Hours, when he awaked much refresh'd.--- The Hiccup never return'd again, but the Man soon recovered.

The next Person to whom I gave this Medicine was a Gentleman, who had been ill eleven or twelve Days of a petechial Fever. I found him delirious, with convulsive Catchings in the Tendons; and an Hiccup very frequent, and so loud, that I very plainly heard it at the Bottom of the Stairs, before I went into his Chamber: His Pulse was, at the same time, very weak, but exceedingly quick. I immediately order'd him a Bolus with ten Grains of Musk mixed with Camphire (e), as in the former Case; and stay'd an Hour by him to see the Effect of it.— In less than two Minutes the Hiccup left him; in about half an Hour his Skin grew moist, and soon after he fell into a sound Sleep. The Hiccup, as I was informed, returned again the next Morning, though with much less Violence; but both that, and the *Subsultus Tendinum* went off intirely, upon a Repetition of the Bolus,

(e) I added the Camphire mixed with Nitre, on account of the Character given of it by Hoffmann; but, upon further Experience, I found the Musk succeed as well without it. Hoffmann, I find, touches upon the Virtues of this Drug, which he considers as a Sedative: ---
*'Sedantia hæc variis modis efficaciam suam perficiunt. --- Quedam
 'Antispasmodica Sulphure quodam tenui vaporoso ex Regno animali;
 'adeoque magis amico effectum persequuntur ut Lumbrici terrestres,
 ' &c. nec non Moschus & Zibethum, quæ in convulsivis ac epilepti-
 'cis Motibus, ac in ipsa etiam chronica Epilepsia, insigni cum Fructu
 'usurpantur.'*

Bolus, and never return'd again ; so that in a few Days he was able to go out of his Chambers.

In these Persons the *Hiccup* was the Symptom that induced me to give the Musk ; but, observing that this Convulsion, which, every one knows, affords but a melancholy Prognostic, was so speedily removed by it ; and that, in the last-mentioned, the *Subsultus Tendinum* also gave Way to it ; I have since order'd it in many other convulsive Cases, and always with Success. I shall mention two or three.—

About two Months ago, I was called to a Daughter of one Mr. *Carter*, Haberdasher of Small-Wares in *Bewdley*. She was about seven Years of Age, and had been ill of a Fever (proceeding, as it was imagined from Worms) for about four Weeks. At the time I saw her, she had been senseless and speechless for several Days ; as I remember, not less than ten or twelve. A great Part of this Time she had had strong Convulsions, and could scarce swallow any thing at all ; so that they every Hour expected her Death. Mr. *M* —, the Apothecary, told me, That, the Day before I saw her, she had had a general *Tetanus*, her Joints being quite inflexible : When I saw her, every Muscle was, by turns, convuls'd ; her Head, in particular, was so forcibly drawn backwards, as, at different times, to raise her Body from off the Bed. — As she swallow'd with so much Difficulty, there was little Hope of giving her any Medicine by the Mouth ; I therefore ordered twelve Grains of Musk, ground with Oil and Sugar to be mix'd with some thin Broth, and thrown up by way of Clyster ; and this to be repeated at two or three Hours Distance. In an Hour's time after the first Clyster, the Convulsions, as I was inform'd, manifestly abated, and

and the second intirely removed them. After this the Child gradually recovered its Understanding and Speech, and is now perfectly well.

About the same Time, I was called to a young Girl in this Town, about ten Years of Age, who had been seized with Convulsions about three Days before. At the Coming on of the Fit, she usually complain'd of a violent Pain in her Belly and Loins, which, she said, seem'd to draw her Bowels on an Heap: After this, she soon lost her Senses, and was variously convuls'd; not much unlike one in an epileptic Fit. When the Violence of the Paroxysm began to abate, and she a little recovered her Senses, she likened the Pain to a Sword running through her. From the first Seizure, she had seldom had an Hour's Interval betwixt the Fits.—I order'd twelve Grains of Musk to be given her in a Clyster, which was thrown up in the Decline of the Paroxysm: Upon this she instantly recover'd, and has never been so affected since; proper Care being taken to remove the *Stimulus* which occasioned these Spasms.

Mr. *Malpas*, whom I have mentioned already, tells me, That, since he has seen the good Effects of Musk, he has twice tried it himself with great Success. — A Gentleman's Groom, having been kick'd by a Horse on the Pit of the Stomach, was thereby immediately thrown into a violent Hiccup, with great Difficulty of Breathing, and incessant Reachings to vomit, the Stomach and Diaphragm being greatly convulsed. After Bleeding, &c. the Musk removed the Hiccups, &c. immediately.—The other Case is more remarkable: A Farmer's Wife in the Country, as she sat at Table, was suddenly seiz'd with an Inability

ity of swallowing. She said, When the Meat or Drink came to a certain Part of the Gullet, she was immediately affected with the Sensation of a tight Cord round her Neck, which seemed almost to strangle her, so that she was obliged to throw her Food back again. — As this was manifestly a Spasm of the *Oesophagus* and Muscles of Deglutition, he order'd her our Antispasmodic, mixing two Scruples of Musk (as I remember) in a six-ounce Phial, and order'd her to take this by Spoonful. Several of the first Doses she rejected, as she did other Liquids; but, perceiving that the Disorder gradually abated upon the Use of this Medicine, she persever'd in it, and, at last, could swallow freely and easily as before. This single Bottle completed the Cure.

I could cite many more Instances to the same Purpose, both from my own Practice, and that of the other Physicians of this Place, to whom I communicated my Observations. One I cannot omit, which my very learned and ingenious Friend Dr. *Mackenzie* has furnish'd me with. I beg Leave to transcribe his own Words:--- ‘ Musk, (says he) given, according
‘ to your Method, in a proper Dose, that is, from six
‘ Grains to sixteen, is the best antispasmodic Medicine that I know. The Efficacy of it in Hiccups,
‘ arising from any nervous Disorder, (and not from
‘ any Wound or Inflammation) is surprisingly great
‘ and sudden. Of many Instances I have seen, I shall
‘ only mention one:

‘ A young Lady, reduced to an extreme Degree
‘ of Weakness, by a slow wasting Fever, attended
‘ with a great Disorder and Lowness of Spirits, had,
‘ when I first saw her, the most frequent Hiccup I
‘ ever

‘ ever heard; it returning with every Inspiration. I
 ‘ order’d her a Bolus with six Grains of Musk, and
 ‘ the Apothecary stay’d to give it. The Moment she
 ‘ had swallowed it, she screamed out so violently,
 ‘ that she alarmed the whole House, and rais’d such
 ‘ an Outcry, that Mr. *Holyoake*, of *Henly* in *War-*
 ‘ *wicksbire*, the Apothecary, with all his good Sense,
 ‘ found it difficult to compose the Tumult. From
 ‘ screaming she fell into a laughing Fit: Soon after
 ‘ she grew calm, and fell into a Sleep, which lasted
 ‘ several Hours. The Hiccup ceased from the Mo-
 ‘ ment she took the Bolus: It returned some Weeks
 ‘ after from taking Cold, but was quickly removed
 ‘ by the same Medicine.’

Those who are acquainted with the learned *Hoff-*
mann’s Writings, well know of what extensive Use
 Antispasmodics are; and the Instances here given, are,
 as I imagine, sufficient to prove, that *Musk* is a Me-
 dicine of *uncommon Efficacy* in convulsive Disorders.
 I can boldly affirm, that, when given in its due Dose,
 it has never once fail’d my Expectations. I have
 sometimes indeed been obliged to repeat the Doses
 three or four times; but it has always answer’d at
 last, in all Cases where I had Reason to expect it
 should. ----- It is not to be imagined, that where the
 Spasms proceed from any *Stimulus* fixed in a certain
 Part, that *This* or any other Medicine can absolutely
 cure the Convulsion, if the Cause be not removed (*f*);
 yet

(*f*) The celebrated *Hoffmann’s* Rules, regarding the Use of Spe-
 cifics, are most certainly just, and must necessarily hold in respect to
This, as well as any other: ‘ *Movendum quod quum hæc ipsa* (1c.
specifica

yet I think it appears, from some of the foregoing Histories, that, even without That, it may greatly alleviate them, and gain Time for other Remedies.

Under the Quantity of six Grains I never found much Effect from it; but it succeeds best, when given to ten, and upwards. In the larger Quantities, it never fails to produce a mild *Diaphoresis*, without at all heating, or giving any Uneasiness to the Patient (how much soever it has been decried by some Writers on these Accounts): On the contrary, it raises his Spirits, and eases his Pains. After the Sweat breaks out, he commonly falls into a sweet refreshing Sleep. Some Persons I have given it to have observed, that their Sweat is affected with the Scent of

‘ *specifica Remedia*) neutiquam Viribus polleant absolutis, sed relati-
 ‘ vis, & certis Conditionibus ac Circumstantiis limitatis, in eorum Usu
 ‘ harum imprimis exquisita habenda sit Ratio, quo speratus respondeat
 ‘ Effectus. Deinceps singularis Efficaciae haec quidem recte usurpata
 ‘ sunt Remedia; sed nihil certe juvant, nisi prius Corpus fuerit prae-
 ‘ paratum; & Obstacula, quae Virtutem infringere aut impedire possunt,
 ‘ remota. Hinc quando Sanguis in Venis redundat, & primæ Viæ Sen-
 ‘ tina Sordium obsessæ, necesse est, ut Sanguis prius subtrahatur, &
 ‘ prima Corporis Regio a crudorum, biliosorum, recrementitiorum Suc-
 ‘ corum Saburra repurgetur ac depleatur. ** Denique in Specificorum Usu
 ‘ accommodo servanda Methodus; ita quidem, ut Artifex non solum
 ‘ Tempus, Dosis, ac Regimen teneat; sed etiam quamdiu iisdem insisten-
 ‘ dum, & quæ Victus ac Vivendi Ratio, simul iungenda, exacte calleat.
 ‘ Etenim medendi Methodus specialissima, quæ unice in Medentis Pru-
 ‘ dentia, Judio, diligenti Attentione, Meditatione, & Observatione
 ‘ Naturæ Agrotantis consistit, tanti ad ipsum Effectum est Momenti,
 ‘ ut ipsi Specificis & appropriatis solis longe sit anteposenda; & sine
 ‘ ea, Remedia quantumvis egregia, alienæ & infructuosæ sint Opera-
 ‘ tionis; nec quicquam solidi in Arte præstare possint. Et hæc qui-
 ‘ cunque studiose observat omnia, is certe desiderato potietur Fine, quan-
 ‘ tum scilicet malo plus valet Ars nostra salutaris.

Med. rational. Systemat. Tom. 3. Sect. 2. Cap. 8.

of Musk. ---- When it is taken in the Quantities and Manner order'd in the Prescription mentioned by Dr. *James*, the Sweat it procures is very copious, and seldom goes off under thirty or forty Hours; yet, tho' it continues so long, it gives not much Fatigue to the Patient; on account of the easy Sleep and Spirits which the Medicine gives at the same time. In these respects, the Operation of Musk much resembles that of *Opium*; but is, in This, much preferable, that it leaves not behind it any *Stupor* or Languidness, which the latter often does; so that it seems rather to approach what is said of *Oleum animale*. Musk, therefore, seems likely to answer in those low Cases where Sleep is much wanted, and Opiates are improper.

I always chuse to give it in a Bolus, if that Form be not disagreeable to the Patient; because the Perfume in that Way is not near so strong as in any other: Indeed when given in a large Quantity, it is rather fetid than fragrant; and I never yet met with any hysterical Person, how averse soever to Perfumes, but could take it in that Form without Inconvenience.

I cannot deny but I have once or twice prescribed Musk, and not found the usual Success after repeated Doses; but this, upon a more exact Scrutiny, I found was owing only to some Sophistication of that which had been used: For, after having tried three or four Doses from one Shop without Success, upon sending for the same Quantity as had been before order'd, from another Person, whose Musk I had before experimented, I found the very first Dose answer as usual. ---- Indeed it is much to be lamented, that a
Medicine

Medicine of this Consequence should be so liable to Adulterations, and the *Criteria* of its Genuineness so ill settled. It is therefore to be wish'd, that some Gentleman, who has Skill and Leisure enough, would oblige the World with an accurate *Analysis* of it, and settle the Characteristics of that which is genuine ; discovering, at the same time, the Frauds and Artifices, by which it is adulterated and counterfeited, and a certain Method of detecting them : For, where the Musk is not genuine, all that is hitherto said will prove but of little Service, either to the Patient or Physician.

Worcester, Oct. 21,
1744.

J. Wall.

II.

A Letter from Alex. Reid, Esq; to Dr. Wilmot, concerning the Effects of the Tonquinese Medicine.

Dear Sir,

London, Nov. 5. 1745.

AS your Zeal and Abilities for promoting the Good of Mankind are my principal Inducements, they must be my only Apology, for troubling you with the following Account of what I know concerning the internal Use of Musk in large Quantities.

About 15 Years ago, I learn'd in *China*, that the *Tonquinese* had an infallible Cure for the Bite of a mad Dog ; and, being very desirous of possessing so

valuable a *Recipe*, I was, two or three Years after, favoured with it by the late Mr. *Hart*.

They take of the best Musk about sixteen Grains; of the purest native Cinnabar, and finest Vermilion, each about twenty-four Grains; and, having reduced them separately to impalpable Powders, mix and administer them in about a Gill of Arrack; which, in two or three Hours, generally throws the Patient into a sound Sleep, and Perspiration; if not, they repeat the Dose, and think the Cure certain.

As I had no room to doubt the Fact, I began to consider attentively the Symptoms of the Distemper, and the Nature of the Remedy. The former seem'd to proceed immediately from the Irritations of the Nerves by the Acrimony of the Juices; which, being constantly and violently hurried about, are, by that Motion, and the Heat attending it, broken, colliquated, and gradually rendered rancid, putrid, corrosive, and even caustic: In the mean time, the Nerves, being more and more vellicated by the increasing Sharpness of the Humours, become proportionably more rigid and constricted; at once augmenting the Velocity of the Blood, and shutting up all the Pores and Passages of the natural Excretions and Secretions; while what should, but cannot, pass off by them, exasperates the Disorder, till the Juices become so corrosive and caustic, as to produce mortal Convulsions.

Believing this Theory to be just, so far as it goes, I readily concluded, that a Medicine capable of relaxing the nervous System could not fail of relieving it from the above-mention'd Effects of Irritation, and thereby putting a Stop to Convulsions, opening the constricted Passages of Nature, moderating the Velocity

city of the Blood, and procuring Sleep; imagining also, that, by the same soothing Quality, the Juices themselves might, not improbably, be rendered more mild and innocent when impregnated with the Medicine. And such a Medicine I judged Musk to be, on account of its known, and almost instantaneous, Effects on Persons of a lax Habit; whose Nerves are so suddenly slacken'd, and the Motion of their Blood so diminished by the least Smell of it, that many of them faint away: Besides, its Odour is so exceedingly subtil, as to penetrate thro' the closest Substances; and may therefore be supposed easily to pervade the minutest Vessels of the human Body, and to diffuse its softening balsamic Virtue thro' all the Juices thereof.

The Arrack seem'd also a very proper Vehicle for the Musk; not only as they make together a very agreeable Bitter, but also because inflammable Spirits resist Putrefaction, and also, in some measure, coagulate animal Juices, which are not already corrupted; by which Effects the too much rarefied Blood is condensed, and hindered from putrefying further; while the bad Juices, being separated from the sound, are plentifully thrown off by the Passages, which the Musk has relaxed, and opened for them. The native Cinnabar seem'd to be sufficiently recommended by its known Uses in Physic, against Acrimony, Obstructions, and Convulsions: But of the Vermilion I can only say, That tho' it be a Preparation of the former, yet, as the *Tonquinese* seem to think its Virtue different, it were to be wished, that we knew their Method of preparing it, in which they certainly excel.

After

After I had long considered and examined these Principles in my Mind, I satisfied myself, that they might justly be applied to many other Cases; and that the Medicine would be of especial Service in malignant putrid Fevers, and Convulsions: And having, in the Year 1739. contracted with the Government for the Transportation of Convicts, I communicated my Opinion to Mr. *David Ross*, an ingenious and sensible Surgeon, whom I had employed to take care of their Healths; and prevail'd on him to make the Experiment in a very desperate Case; for I did not care to venture on any other. Please, in the mean time, to observe, that, as *Tonquin Vermilion* was not to be had, I substituted an equal Quantity of factitious Cinnabar in its stead, and sometimes gave Rum or Brandy instead of Arrack; in other Things I generally adher'd to the original Prescription.

CASE I.

In *December* 1739. two Convicts in *Newgate* were at the same time very ill of the putrid, infectious, malignant Fever, commonly call'd the *Gaol Distemper*. All the usual Methods of Practice having been tried in vain, their Condition appear'd to be quite desperate. One of them died in the Evening, and the other was not expected to survive till Morning, being covered with flat petechial Spots, and delirious. Mr. *Ross* therefore administered to him the above described Medicine about 9 or 10 o' Clock at Night; and, next Morning, to his great Surprise, found him quite free from the Fever, eat-
ing

ing Water-gruel, and crying out for Meat, after having slept well, and perspired plentifully. The Spots on his Skin rose, and the next Day scaled off.

CASE II.

Soon after this, a Convict, who had lived in good Credit, laid his Condition so much to Heart, and drank so freely of spirituous Liquors to drown his Care, that he fell into a violent Fever. He was on the Master's Side, where his Relations look'd after him; so that I heard nothing of his Case till it was very desperate. He was delirious to a high degree, and had Catchings in his Hands and Face. He took the above Medicine at Night, slept and perspired well, and next Morning waked intirely free from his Distemper, excepting that he had such a Tremor left in his Hands, that he could not carry a Glass to his Head; on which account I order'd him a second Dose, and he was perfectly cured.

Encouraged by these Successes, we administer'd the Medicine to a great many other Transports, who had the *Gaol Distemper*; and generally found it to have the same salutary Effects; more especially where the Patients were delirious or convulsed; as can be attested by Mr. *Louttil*, Apothecary, who made up the Medicines for my Surgeons; and was himself a Witness of several surprising Cures perform'd by it: Nor did I ever hear of any bad Effects from it.

CASE III.

Mr. *Ross*, the Surgeon above-mentioned, having caught the *Gaol Distemper*, by attending those who were sick of it on board, came ashore at *Gravesend*, and desired me to provide another Surgeon; for that he was so ill he could not go the Voyage. He was blooded, and took 16 Grains of Musk in a Glass of Rum, without the Cinnabars, which were not to be had ashore, nor easily to be fetch'd from on board. This was at Night; he slept, perspir'd, and waked about 10 in the Morning so well, that he went on board directly, and continued his Voyage.

CASE IV.

Encouraged by many Instances of the first Case, I ventured, about three Years ago, to give half the *Tonquin Dose* (but without the Spirits) to my own Child, then about three Years old, who was seized with the Small-Pox and Convulsions. She slept sound, and perspired plentifully after it: The Small-Pox rose kindly, and she did very well, having never had a single Fit of Convulsion since, though extremely subject to frequent and dangerous ones before.

CASE V.

About two Years ago, Mr. *Ross* gave much the same Dose to my Servant's Child, who was then about eight Months old, and had Catchings in her Hands, occasion'd by a violent Teeth-Fever. She slept, perspired, and waked perfectly well.

CASE

CASE VI.

About the same time, Mr. *Gordon*, a Clergyman, who lived near *Greenwich*, having come from thence to *London* with a Fever upon him, became quite delirious, if he was not so before he set out. Mr. *Ross* gave him the *Tonquin* Remedy, which made him sleep and perspire, so that by next Morning he was perfectly cured.

CASE VII.

A Lady of my Acquaintance, being very subject to violent hysterical Convulsions, was seized with a Fit, as I sat at Supper with her about three Years ago, by which every Part of her Body was terribly agitated. Happening to have in my Pocket a Bolus made of the Musk and Cinnabars, I bruised it in the Palm of my Hand, and clapp'd it to her Nose, which suddenly relieved her. I left it with her, and she has since told me, that she never goes abroad, nor to Bed, without it.

CASE VIII.

Observing the Efficacy of this Medicine in curing *Deliriums*, I conceived that it would be of Use against Maniacal Distempers; and happened, about two Half-years ago, to say so, in the Presence of a Gentleman of *Oxford*, whose Son had been, for some time, exceedingly disordered in his Senses, by a Disappointment in Love; being unable to sleep, refusing Sustenance, and attempting to throw himself out of the Windows of a high Room where

he was confin'd. The Father begg'd me to give him the *Recipe*, and assur'd me he would make use of it, as the Methods formerly tried had proved unsuccessful. He soon return'd me a Letter of Thanks, acquainting me, That the Medicine had made his Son sleep sound for 23 Hours, that he had perspired plentifully, and wak'd in his Senses. I have since heard, that he continues well, and, from a Skeleton, is grown fat.

CASE IX.

A particular Friend of mine went mad about a Year and half ago, by too intense Thinking. I mention'd the preceding Case to Dr. *Armstrong* and Mr. *Ferguson*, who attended him; and, with their Approbation, gave him Musk, native and factitious Cinnamon, of each a Scruple, in about a Gill of Arrack. In about three Hours, he fell, or seem'd to fall, asleep; upon which, supposing the Medicine had taken Effect, we left him: But, soon after we were gone, he wak'd; and, next Day, seeming very little, if at all, better, was removed to a private Mad-house. There nothing else was done to him; but at Night he slept tolerably well, appeared much better next Day, and continued mending, till he was in a little time quite well, as he is now. How much of this Cure may be attributed to the Medicine I do not know, as it did not operate immediately, nor in the usual Manner. I mention it chiefly to shew, that even twenty Grains of Musk had no *bad* Effect upon him, if they had not a *good* one.

CASE X.

Not long after, Mr. *Louttit* and I gave the same Dose to a Gentlewoman, whose Brain had long been turn'd by religious Terrors, which first affected her about the Time that her *Menses* ceased. I was, at first, surpris'd to find her suddenly become quite gentle, obliging, and reasonable; but these good Effects, as she slept but little, went off next Day, and she soon appeared neither better nor worse than she was before; and in the same Condition she still continues: So that neither in this Case did the large Dose of Musk do any Mischief.

These, Sir, are some of the many Experiments made with the above recited Prescription by myself, and the Surgeons under my Direction. You may depend on the Facts as near as I can recollect them; and if you desire to be further satisfied, Mr. *Louttit*, my Apothecary, and Mr. *Mackenzie*, one of my Surgeons, now in Town, shall wait upon you. Give me Leave only to observe, that, where I thought the Case required it, I have given as far as 24 Grains of Musk to Convicts, and never found any ill Effects from it, tho', on some Occasions, it disappointed my Hopes. Whether there was any Error in altering the *Tonquinese* Proportions, the Cause of which I cannot recollect, or whether the Medicine would have succeeded in the Instances where it miscarried, had the original Prescription been kept to, and repeated as directed, I leave to your Judgment and Experience.

But I cannot help asking your Opinion and begging you to consider, Whether, upon the Whole, the Virtues of this Medicine may not reasonably be thought to extend to many other Cases; and particularly to the Epilepsy and Plague. But, whatever Judgment you may form of the Medicine which common Good-will to my Fellow-creatures obliges me to recommend to your Thoughts, I intreat, and hope, you will pardon my Presumption in going so far beyond my own Province; seeing I shall always be, with the highest Esteem, and sincerest Respect,

Dear Sir,

Your most obliged, and

and most obedient Servant,

Andr. Reid.

XIX. De Planta minus cognita, & hætenus non descripta, Commentarius: Auctore *Gulielmo Watson*, REGIÆ SOCIETATIS Sodale.

GEASTER *Volvæ Radiis & Operculo elevatis.*

*Read in English
Dec. 20. 1744.
here printed with
Alterations.*

GEASTER appellatur plantarum genus a *Michelio* constructum; cujus quinque species accuratissimus idem auctor detexit, atque in tractatu suo de novis plantarum generibus

generibus adumbravit. Denominatur autem ex idiomate Græco (γῆ terra, & ἀστὴρ stella), propter angulos quosdam acutiores e centro radiatim excurrentes in omnibus hujus plantæ speciebus, eo ritu, quo stellarum coruscationes in pictura effinguntur.

Cum Lycoperdo (a) *Tournefortii* & (b) *Linnei*, vel Bovista (c) *Dillenii*, cum (d) Lycoperdoide, Lycoperdalstro, & (e) Carpobolo *Michelii*, proximam affinitatem obtinet hocce genus. (f) *Linnaeus* etiam, necnon in generibus a *Linnaeo* datis (laudabili sane instituto) sectator ejus (g) *Royenus*, omnes has plantas sub Lycoperdi nomine generico complexi sunt. Salva tamen reverentia, qua eximios istos in arte botanica viros colam, quorum legibus, in omnium fere plantarum generibus a sola fructificatione firmandis, licet in cæteris partibus minime convenient, arcte adhæreo; in multis certe tam ex *Linnei* Cryptogamiis, quam ex *Royeni* Cryptantheris, ad habitum plantarum, etiam in generibus constituendis, spectare oportet; idque in Fungis, Fucis, Algis, forsitan & in Capillaribus, maxime necessarium esse videtur.

Ad descriptionem mirificæ hujus plantæ concinnandam, in tres partes distrahere lubuit, Volvam scilicet, Operculum, & Fructificationem; quas seorsim tradere nunc aggrediar.

Volva (ut non cum *Plinio* tantum, sed & *Clusio*, aliisque recentioribus loquar) concava est, calycis formam repræsentans, cujus diameter sesquipollicem æquat. Lata huic calyci adest basis, e centro cujus, dum

(a) Instit. R. Herb. p. 562. (b) Linn. Gen. Plant. p. 510.
 (c) Cat. Giff. p. 196. (d) Michel. nova Plant. Gen. p. 221. (e) Ib. d.
 (f) Linn. Gen. 510. H. Cliffort. p. 479. (g) Flor. Leydenf.
 Prod. p. 518.

dum adhuc planta adolescat, radículas fibrosas, raras tamen more gentis, emittit, eidem nutrimentum subministrantes. Cum vero toti huic stirpium familiæ humiditas minime sit grata, radiculæ una cum volvæ centro, cui erant infixæ, antequam ad maturitatem perveniat planta, marcescunt; qua de causa in basis fundo foramen conspicitur. Volva autem, postquam ad tertiam pollicis partem surrexerit, fit quadrifida, laciniis gaudens obtuse lanceolatis, ad apices parum reflexis, ad margines vero integris. Huic volvæ vis elastica tribuitur; color externus est cinereus cum superficie inæquali, internus vero lævis & albicans.

A singulis volvæ laciniarum reflexarum apicibus pars plantæ assurgit, quam operculum voco. Ab hoc a commissuris cum volva sensim se extendente, bini conflantur fornices ad angulos rectos intercurrentes, quorum altitudo a volvæ laciniis sesquipollicem parum superat. Operculi densitas & crassitudo corticem Cinnamomi æmulantur; ad margines nonnihil sese invertit; color introrsum albicat, extrorsum flavo rubescit. His addatur, quod membrana quædam fusca, impariter quadripartita, fornicis utriusque culmini adhæret.

Ex operculi centro jam descripti, pediculo quartam circiter pollicis partem alto sustentatum eminet pericarpium, oblate globosum, colore fuscum, ad pollicis duas partes tertias latum, capitello papaverino æmulum. Foramen circulare hujus pericarpii summum culmen occupat, in quo cernuntur lamellæ quædam per longitudinem totam intus parietibus pericarpii adnatæ, materia lanuginosa quadam confertæ, quibus tanquam placentis adhærent sub pulveris subtilissimi forma, semina numerosa.

Multis forte rei herbariæ scientia imbutis cognitu difficillimum videatur, quibus modis, dum planta adhuc vigeat, operculo & pericarpio idoneum suppeditatur nutrimentum. His igitur notatu dignum sit, quod in plantæ statu teneriore, volva & operculum cum radiis suis humi procumbunt, piscibus stellæ marinæ dictis haud absimilia, materiaque quadam gelatinosa simul conjuncta, cujus ope una cum pericarpio ac semine nutriuntur. In hoc situ dantur omnes Geastri species a laudato *Michelio* propositæ; pariter ac *Fungus Crepitus Lupi dictus coronatus & inferne stellarus*, ab egregio (a) *Raio* nostro primum memoratus, cujus in tertia Synopseos Stirpium Britannicarum (b) editione icon exhibetur. Maturo autem semine, gelatinosa materia quæ volvam & operculum intercurrit jam arefcit. Hinc rigida fiunt & elastica; quæ de causa, si contactus ad laciniarum apices demas, divisa cernuntur. His ita effectis consurgunt sensim volvæ radii, operculum, & fructus, turriculamque fornicatum tota planta proxime repræsentat. Hic ratiocinandi modus (crescentem enim perscrutandi facultas nemini adhuc fuit concessa) vix ullum dubium admittit; cum non sabulum tantum, sed etiam lapillum, hoc ipso tempore conspicuum, completitur volvæ tunica exterior rugis obsita. Quin auctoritate etiam *Micheliana* certiôres facti sumus, (c) *Carpobolum* (Geastro, ut supra dixi, valde affinem) non operculum tantum ex cavo ad convexum sublevare posse, momento vero temporis ea violentia hoc effici, ut fructum suum parvum ac globosum in altum ejaculetur.

Hæjus

(a) *Raii* Synopf. Edit. 2d. p. 16.
nova Plant. Gen. TAB. 101.

(b) TAB. I.

(c) *Michelii*

Hujus plantæ exemplaria duo, nec plura videre contigit, communicavit mecum amicitia conjunctissimus *Robertus Nicholls* pharmacopæus *Londinensis*, primi subfellii botanicus; cujus non tantum ardorem, sed etiam ingenium scientiam naturalem promovendo, multoties ipse fui expertus. Majus horum, haud procul a *Reading*, — *Merrick*, M. D. collegit; minus vero prope *Wickham* in agro *Cantiano* nascebatur. Sub *Martii* mensis finem ambo provenerunt.

TAB. II. *Fig. 11.*

- A.* Pericarpium.
- B.* Operculum.
- C.* Volva.

Fig. 12.

- Pericarpium antice visum; cujus
- A.* Foramen circulare.

E R R A T A.

In *Page* 198, *Line* 13. for *Abductores* read *Adductores*. *P.* 172, *l.* 28. for *b*, read *ib.* *P.* 200, for *XV.* read *XVI.* *P.* 225, for *Alex.* read *Andr.*

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ERRATA.

- No. 474. In the Contents *Art. IX.* and in p. 166. for *Wintler* read *Winkler*.
- Ibid.* p. 166. l. *penult.* N. B. TAB. II. Fig. 4. refers to a Figure in the Author's original Book.
- Ibid.* p. 201. The Draught of the Roman Inscription, there mentioned, is said to be taken by a Scale of *one Fourth* of the Original; but *ibid.* in TAB. II. Fig. 9. it is reduced to near *one Sixth*.
- No. 475. p. 311. l. 1. after *Vas vitreum* add Fig. 5.

I. Sam. Christiani Hollmanni, *Philosoph. in Reg. Georgia Augusta [Gottingenf.] Prof. Pub. Ord. ad Cromwellum Mortimerum, M. D. R. S. Secret. Epistola de subitanea Congelatione, de Igne electrico, de Micrometro Microscopio applicando.*

Read at a Meeting of the Royal Society, Jan. 10. 1744-5.

QUUM quas de barometris aliquo abhinc tempore ad te perscripsi, vir clarissime, observationes, tibi, illustrissimæque Societati Regiæ, non displicuisse intelligerem; alia nonnulla, in quæ ab illo tempore incidi, hac, quæ mihi jam offertur, occasione ejusdem illustris Societati Regiæ judicio permittere constitui.

Legi N^o. 418. Transactionum, p. 79. seq. a cl. *Trievaldo* relatum phænomenon, quod adeo mirabile, quum primum legerem, visum fuit, ut, nisi aliunde jam constitisset, congelationes omnes fere in puncto temporis fieri, fidem fere omnem videri potuisset superare. Non absimile vero circa finem anni 1742. mihi contigit, quod, casu fortuito primum enatum, postea, & eadem adhuc hyeme, & proxime elapsa hujus anni, ad certam & constantem experimenti legem revocare studui. Hærebant quippe circa finem dicti anni 1742. quum die 24. *Decemb.* [st. n.] ingens apud nos frigus repente esset exortum, in duobus vitris conicis diaboli, quos vocant *Cartesiani*, figuræ & coloris varii, quibus proximis antediebus ad experimenta quædam usus erat; alteri quidem aqua specificè leviores, alteri vero ab indita

quadam illorum cavitatibus aquæ quantitate majori graviores eum in finem reddiri, quo antliæ pneumaticæ sub campana impositi aëreque ex hac subduſto, aſcendere ſua quaſi ſponte in aqua circumjacente poſſent. Utrumque, in cujus aqua hærebant, vitrum in conclavi quodam frigido, apertum, & phiala quadam vitrea ſolum obtectum erat repositum; ex quo, quum mane d. 24. *Dec.* eadem forte conſpicerem, in calidum ſtatim hypocaustum illa transfero, atque ciſtæ lignæ ex nuce juglandæ fabrefactæ impono, aqua fluidiſſima & pellucidiffima adhuc plena. Quoniam paulo vero poſt in mentem mihi veniebat, recordari, quod vitra, & corpora ejuſmodi quæcunque frigida, in calidum tranſlata locum, inſignem roris in rivulos aqueos ex ſuperficie plerumque deſluentis copiam ſoleant concipere; a damno, vel macula ſactem, ciſtam meam deſenſurus chartam, quam vitris ſubſternam, circumſpicio, dumque alterum vitrum tollo, rore, ſi ita modo appellare fas eſt, multo perſuſum jam invenio; alterum vero, in cujus fundo diaboli-aqua graviores hærebant, ſiccum equidem, ſed totum fere glacie repletum deprehendo. Stuporem fere mihi incutiebat inſolitum & inexpectatum ſpectaculum; de icunculis tamen meis vitreis ſolicitus, tubulum, qui ad manus erat, æneum ante omnia arripio, e. media, ſi fieri poſſet, glacie eaſdem ſervaturus. Quum lamellas vero glaciales, mirifice ſibi intertextas, tenuiſſimas & molliſſimas eſſe deprehenderem, totuſque pro vitri figura conus glacialis vitro circumquaque nullibi cohæreret, quin in pauculâ potius, quæ ipſum circumdabat, & in fundo quoque vitri ad digiti altitudinem reſtabat, aquâ nataret, & ad latera jam ſenſibiliter in aquam reſolvi inciperet; eo ſecurius

rius lætiusque jam jucundissimo hoc spectaculo frui cœpi. Erant singulæ lamellæ glaciales, quæ totum istum truncatum conum constituiebant, tenuissimæ, & vitri fere instar pellucidæ; propterque varie ad se inclinatum situm, ad totius vitri conversionem instar tabularum vitrearum mirifice resplendescabant; quam spectaculi jucunditatem illæ, quæ circa diaboli *Cartesiani* nigerrimi caput hærebant, non parum adhuc augebant.

Hac peropportuna ergo jam uti occasione volebam; atque annon, glacie iterum soluta, & vitro priori suo loco reposito, reproduci idem phænomenon posset, tentare. In ea vero, quum cl. *Triewaldi* experimentum primum legerem, opinione fere eram, quod vesicæ, quæ vitro ejus circumligata erat, pressione, forsan pressioni a frigore oriundæ, gradus auctus, indeque subitaneus ille congelationis effectus ortus fuerit, eademque adeo via in aqua, sufficienter jam frigefacta, etiam mihi rem successuram sperabam.

Abuterer vero tua, vir clarissime, & illustris Societatis, patientia, si omnia, eam in rem, die 24, 26, & 27 *Dec.* instituta, multaque cum molestia sæpius repetita, experimenta enarrare hic singula vellem.

Successit, fateor, experimentum aliquoties; sæpius tamen omnibus licet fere circumstantiis iisdem, spe omni frustratus sum. Fuit [*a*] *integrum* vitrum aliquoties, me inspiciente, glacie repletum; aliquoties [*b*] quasi *cistæ* quædam *glaciales*, me inspiciente & vesicam vitri comprimente, ad latera vitri exortæ sunt, quæ in *crustam* glaciacem, integram superficiem vitri interiorem ambiente, mox abierunt; reliqua aquæ massâ interiori, axemque vitri proxime circumdante, remanente fluida; aliquando [*c*] *singulæ*

Lamellæ glaciales *pellucidissimæ*, & *nivis* fere referentes figuram, in media aqua limpidissima, vitrum me manu tenente & plena die iterum inspiciente, in conspectum subito prodierunt, quæ adeo tenues vero erant, ut manu paullulum tremente in aqua simul titubarent, & contremiscerent, &c. &c. Sed hæc omnia percensere infinitum foret. Ergo summa solum experimentorum, sæpe studioseque repetitorum, capita & quasi fundamenta præcipua, paucis adhuc indicabo.

I. Quando aqua sufficientem frigoris gradum jam conceperat, sive in calidum transferretur locum, sive vitrum solum manu calida comprehenderetur, aqua ferme omnis in momento fere in glaciem convertebatur. Illud tamen sæpius, faciliusve, quam hoc successit.

II. Nihil referebat, sive vesica vitrum obligatum esset, sive minus; & illo quidem casu, sive illigata vesica digitis premeretur, sive minus. Neque,

III. Quicquam interesse deprehendi, sive diabolus aliquis *Cartesianus*, aut alia ejusmodi icuncula, aquæ inhæreat, sive minus; atque si adest, num in fundo vitri hæreat, an aquæ superficiiei innatet.

IV. Icuncula tamen vitrea quadam in aqua hærente, quotiescunque congelationis initium adspicere mihi licuit, ab aliqua semper ejus parte illa incepit, indeque demum se circumquaque diffudit.

V. Experimentum tunc solum ex voto successit, quando vitrum aqua plenum ita aëri frigido fuit expositum, ut tota aquæ massa *æquabiliter* ab eodem penetrari potuerit. Quodsi,

VI. Vero vel ante fenestram apertam, per quam ventus frigidior irrumpebat, vel in pavimento gypso, atri hic passim moris est, oblecto vitrum collocatum esset;

esset; glaciēs quidem generabatur, sed qualem antea sub N^o [6] p. 241 recensui. Quodsi vero,

VII. Vel ante fenestram apertam, vento non spirante, in fundo ligneo, vel in pavimento gypseo, sed interposita assercula, repositum esset vitrum; fere semper experimentum successit: nisi forsā paulo tardior advenerim, atque vitrum glacie jam totum repletum fuerit. Ut,

VIII. Gradum vero frigoris, ad quem pervenisse aquam oportebat, si momentanea ejusmodi contingere congelatio in loco calido deberet, eo facilius tandem possem observare, vasculum paullo latius myrrhinum aqua replevi, & in eodem cum vitro fundo reposui; diligenterque observavi, quando glaciale cuticulam contrahere eodem contenta aqua incipiebat. Et si non exacte enim congelationes illæ erant isochronæ; sæpius tamen hæc observatio ad feliciter experimenti successum mihi profuit. Per molestum enim & tædiosum absque eo experimenti genus est. Forsan vero,

IX. Conica vitri figura, ob angustius, quo pater, orificium, latioremq; basin, ad vim & frigoris, & caloris, per totam aquæ massam æquabilius diffundendam, non parum quoque confert. Saltem in vitro quocunque cylindrico, ob nimis latum orificium, difficulter experimentum successurum esse, persuasus sum; et si ipse nondum tentavi.

Non licuit mihi postea, per tempestatis statum, eadem hyeme experimenta hæc repetere. Repetii tamen proxime elapsa anni hujus 1744. diebus 6, 7, & seq. *Januarii*, cum eodem successu, iisdemque, quæ modo enarravi, phænomenis.

Causam

Causam, cur aqua ad punctum congelationis fere frigefacta, unico fere momento in innumeras istiusmodi, mireque se decussantes, lamellas glaciales, unum corpus continuum efficientes, abeat, si quo contineatur, vitrum *subito calore* circumfundatur, reddere non auserim. *Aristotelici* forsan suo *antiperistaseos* vocabulo mirifice sibi suisque similibus hic placerent. Sed satis habemus in philosophia vocabulorum conceptu vacuum, quorum ego numerum augere nolim. Percensui phaenomena. Judicium de re ipsa, rei que, si fieri potest, modo *Tuo*, vir clarissime, *illustrissimaeque Societatis* imprimis luminibus permitto.

DE ELECTRICIS experimentis, quæ celeberrimorum virorum, vestratis Dn. *Gray*, & *Parisi* Mr. *Du Fay*, industria mirifice, & supra fidem fere exculta sunt, nescio, an Tibi, vir cl. jam constet, quousque jam apud nos in *Germania* promota sint. Postquam a Mr. *Du Fay* nempe repertum casu erat, ex electrificato humano corpore *scintillas*, si ab alio non electrificato contingatur, erumpere, quæ satis acute pungant, repetita hæc experimenta in academia *Lipsiensi* sunt; atque loco rubi, quo Dn. *Gray* & *Du Fay*, usi erant vitrei, *globus* vitreus, quali cl. *Hauksbee* jam olim ad alia experimenta electrica usus est, adhiberi coepit; hacque occasione observatum est, *tantam ignis vim* corpora electrificata, imprimis animalia & metallica, fundere, quibus non *spiritus vini* solum modice repectus, quod facillime succedit, sed & alia corpora inflammabilia, interque ea *pulvis pyrius, pix, sulphur, & cera sigillatoria*, intensius prius calefacta, incendi possint. Postrema hæc experimenta aliena fide refero; priora vero ista propria mihi jam confirmavit experientia. Globo vitreo per quem axis fer-

reus

reus transit, dum celeriter rotatur, apponitur, quam fieri potest proxime tubus ferreus, ex laminis ferreis, stanno obductis, paratus, diametri unius vel dimidii circiter digiti, longitudinis trium aut quatuor pedum, filamentis, ex serico coeruleo paratis, horizontaliter impositus; & ne globum vitreum inter rotandum tubus temere lædat, extremitati ejus cavæ fasciculos ex variis filorum linearum generibus, nunc nudos, nunc auri argentique lamellis obductos, infero, quorum extremitates dum globum contingunt, præter alia jucunda phænomena, vim omnem adhuc reddunt in tubo cohærente ferreo fortiolem. Tubi hujus alteram extremitatem homo pici, ad duorum triumve circiter digitorum profunditatem cistæ cuidam ligneæ infusæ, insiliens manu apprehendit; tuncque vis electrica per totum ejus corpus ita diffunditur, ut quæcunque ejus pars non corpora solum leviuscula, i. e. foliola auri lamellati, attrahat repellatque alternis vicibus; sed quæcunque etiam, sive rubi istius ferrei, sive hominis electrificati pars, si ab alio non electrificato contingatur, scintillas acute pungentes eructet. Sæpe etiam, si is, qui in pice constitutus electrificatusque est, gladio succinctus fuerit, ex vaginæ extremitate scintillæ vel sua sponte prorumpunt. Teneat homo, in pice constitutus, pileum limbo aureo vel argenteo circumdatum sub brachio; tangat alter non electrificatus limbum istum; & satis acutum ille in brachio sentiet impetum & dolorem. Non electrificatus vero aliquis teneat spiritum vini rectificatissimum, modice tepesactum, in cochleari; & alter electrificatus, digito, clave ferreo, extremitate gladii, scilicet ad superficiem spiritus istius appropinquet, & statim apertam hic flammam concipiet. Teneat electrificatus co-

chleare cum spiritu vini manu sua; accedat quicumque circumstantium, & digitos ad spiritum vini appropinquet, & idem statim sequetur effectus. Constituatur secundus, tertius, quartus scilicet in pice, & omnes vel manus inter se jungant, vel mediante fune, tubo ferreo, &c. inter se conjuncti copulatique sint, eodem modo in *postremo* omnia, ac in *primo*, *secundoque*, succedent. Taceo alia, circa attractionem & repulsionem corporis electrici nuper detecta phenomena. Hoc modo addo, quando globus vitreus rotatur, opus esse, ut sicca, & non nimis calida, manus eidem interea temporis apponatur, ad quam globus leviter teratur. Nihil enim adhuc repertum est, quod pares hic cum humana manu effectus præstet. Globus, quo ad hæc experimenta utor, vix diametri sex digitorum *Rhenanorum* decimalium est; & promississime tamen experimenta omnia, favente inprimis tempestate, eodem succedunt, miraque scintillarum, quam corpora eodem electrica evomunt, vis est. Sunt tamen qui majoris molis globis majora adhuc effici posse contendunt, qualia supra jam recensui. Neque vero, ob temporis penuriam, omnia jam tentare potui, quæ & ab aliis jam tentata sunt, & ipse adhuc mente concepi tentanda. Major, diutiusque perdurans, globo, quam tubo vitreo, excitata vis est.

SI EPISTOLÆ limites excedo, tuaque abutor patientia, veniam dabis, vir doctissime. Ut unicum tamen adhuc adjiciam, quum non æque commoda semper scribendi mihi pateat occasio, non inique proculdubio feres. Vidi in v. cl. *Henr. Bakeri* tractatu; *The Microscope made easy*, &c. *Londini*, a. 1743. altera vice auctius edito, p. 47. quod cl. *Martin* micrometrum quoddam invenerit, ad microscopium quoddam compositum

positum applicandum. Utor ego ab aliquot annis jam alio quodam micrometro, quod ad microscopium, ab ingeniosissimo *Scarleto* vestro elaboratum, applicui, quodque in foco primi vitri ocularis ejusdem microscopii constitui. Est vero illud ex particula subtilissimæ texturæ sericeæ nigræ, quæ in spatiola quadrata minima divisa est, compositum, quam annulo ligneo, vel chartaceo ita supratendo, ut in foco dicti vitri ocularis collocari commodè possit. Sunt equidem, fateor, spatiola illa quadrata, non omnia ejusdem inter se magnitudinis. Præterquam vero, quod hoc propositum ad faciliorem & commodiorem eorundem dinumerationem multum confert, (siquidem, si ejusdem omnia exacte magnitudinis inter se essent, impossibile plane foret, eadem numerare) parum quoque certis inde deducendis conclusionibus idem obstat. Quoties enim cunque 20, 30, 40, & horum spatiorum quadratorum, secundum unam eandemque micrometri, hoc est, telæ istius subtilissimæ, lineam pergendo numeravi, undecunque etiam numerandi in eadem initium fecerim, eademque cum certo quodam, sub microscopio posito, objecto comparavi satis exacte; atque ita numerum quadratorum objecti diametro respondere observavi, ut plerumque vix unum, aut dimidium, spatium istiusmodi quadratum vel abundare, vel deficere deprehenderim; quod in tam incomprehensibili objectorum subtilitate vero negligi tutissime potest.

Quum iterato ergo experimento jam invenissem, N^o 5. microscopii mei *Scarletiani* minimum 27-28, vicibus objecti diametrum augeri; sumsi augmentum ejus 25 modo vicibus respondere, quo certior esse possem, quæ de sequentibus vitris, ope micrometri

mei, invenissem augmenta non majora, sed minora, iusto adhuc esse. Quum hoc ergo modo reperissem, N^o 1. ejusdem *Scarletiani* microscopii objecta minimum 250, vicibus, rationi diametri augeri, atque animalcula feminalia humana, sub eodem visa, vix acari majoris nudo oculo visi, si absque caudula sua ista accipiantur, magnitudinem æquare; per se jam patet ultra 15,625,000, hoc est ultra quindecies millies mille animalcula feminalia humana unius acari spatio comprehendi posse. Et multo tamen exiliora his animalculis in aqua, piperi rotundo vel fœno etiam vulgari, affusa, præter illa sæpe observavi, quæ post aliquot dies in eadem in conspectum plerumque veniunt. Ejusdem etiam micrometri ope in duplicem modum incidi, animalculorum feminalium quantitatem in lacte piscis, multo, quam a *Leeuwenhoekeo* quidem factum est, accuratius determinandi; de quo alia forsitan occasione pluribus. Illud unicum modo, ne nimium tua, vir clarissime, humanitate abutar, in præsentii addo, unam lineam decimalem pedis *Rhenani* cubicam, in lacte carpionis, ultra 244,140,625 animalcula feminalia continere; totum vero lac carpionis, nondum duas libras *Norimbergicas* pendentis, quod 1084 granorum erat, 2080 lineas decimales cubicas, ejusdem pedis *Rhenani*, circiter constituisse, uti experimento hydrostatico reperi. Integrum ergo lac illud ultra 507,812,500,000 animalcula feminalia continebat. Quod si dimidium vero lactis istius animalculis solum constare supponamus, dimidium ejus vero fluidum quoddam, in quo ista degant, esse, quod iusto tamen majus suppositum esse illi omnes facile largientur, qui, quam nihil fere fluidi interlabentis in piscis hujus, aqua nondum diluto, semine obser-

vari possit, attenderunt; animalculorum tantum vivorum in carpionis, qui nondum duas libras *Norimb.* complet semine numerus 253,906,250,000 adhuc erit major: quod certe omnem nostram imaginandivim, minime tamen infiniti Conditoris potentiam, superat.

Sed satis diu jam, vir doctissime, te moratus sum. Nihil ergo adjicio, quam, ut impense te rogem, ne, qua usus sum, prolixitatem inique feras, mihi vero porro faveas. Ita Vale.

*Dabam Gottingæ, in Academia Georgia Augusta,
Die 15. Octob. A. 1744.*

P. S. Ad tantam profunditatem barometrum, quator, ordinarium, dum hæc scribo, delapsus est, ut unica modo ex scala ipsius ordinaria linea ipsi restet. Sed Auster etiam vehementissimus spirat. Hora vero noctis duodecima etiam infra hanc ultimam scalæ suæ lineam Mercurius subsedit.

II. *Some Account of the Gigantic Boy at Willingham near Cambridge.*

1. **T**HE first Notice given to the *Royal Society* of this extraordinary Child, was a Letter communicated by Mr. *Philip Miller*, F. R. S. to the *President*, inclosing one he had received from the Rev. Mr. *Almond*, Minister of the Parish; which was read at a Meeting of the *Society* on Nov. 8. last: At which time was likewise read a Letter from one Mr.

Baily to *James Theobalds*, Esq; *F. R. S.* on the same Subject, and much to the same Purpose.

The following is an Extract of the Rev. Mr. *Almond's* Letter,

S I R,

Willingham, Oct. 3. 1745.

Read Nov. 8. 1744. **I** TAKE the Freedom to give you an Account of a Prodigy in Nature in the Town where I dwell; leaving it to your Discretion, whether it be worth your while to give the *Royal Society* Information thereof.

A Servant, who married out of my Family, was delivered of her second Child, a Son; who, at his Birth, had something very extraordinary about him above other Infants; in particular, *in partibus generationis*; besides uncommonly large in his whole Body.

He grew to Admiration for Three-quarters of a Year, having only the Breast-Sustenance; when his Mother died suddenly; and, as is supposed, he was accessory thereunto, by drawing away her vital Nourishment.

Since her Death, he has continued growing in Proportion: And, altho' now but 2 Years and 11 Months old, is 3 Feet 9 Inches high and better; and every Part in Proportion thereunto: His Strength and Courage such as to overcome Boys of 6, 7, and 8 Years of Age: His Voice, like a Man's, very groom; Weight above Four Stone; and he appears to have as much Understanding as a Boy of 5 or 6 Years old. But, what is most surprising, his *Penis* is Four Inches long when

when erect; the Hair on the *Pubis* an Inch long and thick. The Report of him has brought People far and near to see him.

Your obedient humble Servant,

Edmund Almond.

2. *A Letter from Mr. Tho. Dawkes, Surgeon at Huntingdon, to Dr. Mead, F. R. S. & Med. Reg. concerning the same Child.*

Learned Sir,

*Read Jan. 10.
1744-5.*

A *Prodigy of Nature*, hereunder exactly described, has, for some Months past, engag'd the Attention of the Curious in this Neighbourhood; and seems to demand that of that Learned and Honourable *Society*, of which you are a Member of the first Rank: And as such, tho' a Stranger to you in Person, yet I presume the following Accounts will not be altogether unacceptable to you.

At the Instance of the Midwife concerned at the Time of Labour, I went to a Village called *Willingham* alias *Wivelingham*, about Six Miles North of *Cambridge*, to see a Boy, then (the latter End of *August* last) just Two Years and Ten Months old; of whom I took, with the utmost Accuracy, the following Dimensions; *viz.*

His

	Fect.	Inches.	Tenths.
His <i>Height</i> , which was ———	3	8	5
The Measure round his <i>Neck</i> ; ———	1	1	2
———— of the <i>Waist</i> ———	1	11	2
From the Head of one <i>Humerus</i> to } the other. ——— ———	0	11	8
From the Head of the <i>Humerus</i> to } the <i>Cubit</i> ——— ———	0	8	2
From the <i>Cubit</i> to the <i>Extremity</i> } of the <i>Ulna</i> and <i>Radius</i> ———	0	6	0
The Measure round the <i>Wrist</i> ———	0	6	0
The <i>Length</i> of the <i>Thigh</i> , from the } <i>Head of the Femur</i> to the <i>Head</i> } of the <i>Tibia</i> ——— ———	0	10	8
The Measure round its <i>thickest</i> Part	1	2	3
From the <i>Head</i> of the <i>Tibia</i> to the } <i>Astragalus</i> ——— ———	0	9	1
The <i>Length</i> of the <i>Foot</i> ———	0	6	0
The <i>Breadth</i> of the same ———	0	3	3
The Measure of the <i>thickest</i> Part of } the <i>Calf</i> ——— ———	0	10	3
The <i>Length</i> of the <i>Penis</i> , as pendent	0	3	3
Its <i>Circumference</i> ———	0	2	7
The <i>Testes</i> , measured round to each } Side of the <i>Penis</i> ——— ———	0	5	2

His *Weight*, in his *Cloaths*, was Four Stone and Two Pounds.

The *Hair* of his Head is *long*, *strong*, of a *dark-brown* Colour, and displays itself, *naturally*, in fine *Curls*.

He has the distinguishing Mark of *Puberty*; for the *Lanugo* upon the *Pubes* is as *long*, as *thick*, and as *crisp*, as that of an *adult Person*.

He

He is *very strong*: I saw him take up and throw from him, with much Facility, a *Blacksmith's Hammer*, which weigh'd *Seventeen Pounds*: And when he is provok'd by other Children, (for he goes to School) he does not *fight with his Fist*, nor *Legs*, but *collars* them, and lays them prostrate by mere Strength. Thus, they tell me, he will frequently serve Boys of eight or ten Years of Age.

His Voice, in common, is *extremely manly*, as deep a Bass as you can well conceive; and he pronounces very distinctly.

He has something a little *savage* in his Look; tho', they tell me, he is not naturally quarrelsome.

He eats and drinks with great Moderation.

His Understanding seems to be equal to that of a Child of 5 or 6 Years old.

He is quick of *Apprehension*, and has a very retentive Memory.

He has something of a *stately Walk* with him; and seems conscious of that uncommon Share of Strength which the Author of Nature has endow'd him with.

He is not made a *public Shew* of, but walks about, and plays in common with other Children.

When I saw him, the *Glans of the Penis* was quite uncover'd; and his Aunt, and the Midwife, assured me it was always so.

His *Father* is a *little Man*, a *Labourer*. His *Mother* (who died when he was but 9 Months old; and, as was suppos'd, by giving him *Suck*; for, they tell me, she was found dead with him at her Breast) was a Woman of a *middle Stature*.

The Midwife assur'd me, That, when he was first born, he was no other than what they commonly term,

term, a *lusty Boy*; save that the Parts of Generation were *remarkably large*, and that the *Lanugo* first appear'd when he was near a Year old; which gave great Uneasiness to his Parents, who were very religious People. I am,

Learned Sir,

Huntingdon, Jan. 4.

1744,

Your most Obedient,

Humble Servant,

Tho. Dawkes.

To a second Letter from the same were subjoined the *Affidavits* and *Testimonials* of the Midwife, the Minister, Churchwardens, and others, That this Child, *Tho. Hale*, was born on 31. *Octob.* 1741.

He was assured, by Mr. *Almond*, That, between *Aug.* 28. and *Nov.* 30. 1744. this Child was grown two Inches and an half; *i. e.* from 3 Feet 8 Inches and 5 Tenths, to 3 Feet 11 Inches. C. M.

III. *A Letter from Mr. Browne Langrish, Licent. Coll. Med. Lond. & F. R. S. to the Rev. Dr. Hales, F. R. S. concerning a new Contrivance of applying Receivers to Retorts in Distillation.*

Dear Sir,

Read Jan. 24.

1744-5.

I Return you my hearty Thanks for your kind Present of your Account of some Experi-

Experiments on *Tar-Water*, I think it will be a Means of making it a more useful Medicine: I am sure it will prevent its doing the Mischief I hear it has done in some Cases. For, till the several peculiar Disorders, in which *Tar-Water* may be useful, are found out, the Taking it at random, in such large Quantities, must oftentimes do Hurt.

In Return for your Favour, I have sent inclosed a Scheme, which I have a great while thought of, for the Improvement of Distillation in the chemical Way. I flatter myself, it will be very advantageous in procuring many fine volatile Things, which we cannot so well do in the ordinary Method; and that we may be enabled by it to analyse some Things, as Blood, and such-like Substances, without breaking our Glasses; as the learned *Boerhaave* complain'd he could not do, by reason of a pitchy bituminous Matter rising up into the Neck of the Retort, and bursting it. But, in short, I have found, from many Experiments, That it is the vast Quantity of Air, arising from such Substances, which bursts the Glasses, and which this Method will prevent.

However, I beg the Favour of you to consider it; and to give me your free Thoughts, whether it will be worth while to lay it before the *Society*, in order to communicate it to the World. Your Answer will infinitely oblige,

Dear Sir,

Petersfield, Jan. 10.

1744.

Your most obedient Servant,

B. Langrish.

See TAB. I. *Fig.* I.

- A.* A common Retort in a Sand-Heat.
- B.* The first Receiver, with an Opening at the Top and Bottom.
- C.* A Quart-Bottle fixed to a Neck out of the Bottom of the Receiver; which, being tied on close by means of a Bladder, may be removed at any time, and another instantly placed in its Room; by which means, very little of the Steam will make its Escape: And any Proportion of the volatile Part to be distilled may be saved by itself, without unluting the Recipient from the Retort.
- D.* A second Receiver, inserted into the Opening at the Top of the first, in order to give more Room to the rarefied and new-generated Air, and to receive the most subtile and volatile Parts, which might not be so easily contained in the first Recipient, without great Danger of breaking it, or forcing the Luting.
- E.* A smaller Bottle, for the same Uses as the other. mark'd *C.*
- F.* A Bladder tied on to an Opening. or upper Neck, of the second Recipient; which, as it is much thinner and weaker than any of the Glasses, will always give Way first, and prevent their bursting. Or, wherever the Matter to be distill'd is of such a Nature, that, we are sure, all the Glasses, put together, will not contain the Fumes and Air arising from it; then, if the smallest Pin-hole be made thro' the Top of the Bladder, as soon as the Fumes begin to rise, it will be sufficient to let out the Air as fast as it generated: And, I think, at that great Distance from the Fire, very little, if any thing, but Air, can make its Escape.

Hence

151831.

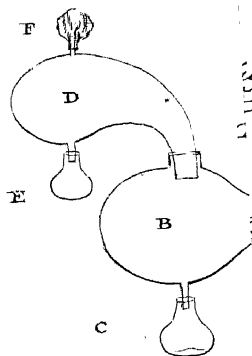
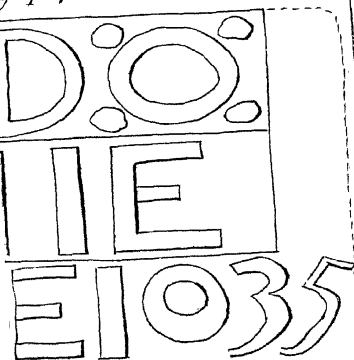


Fig. 4. p. 288.



A Scale of Inches.

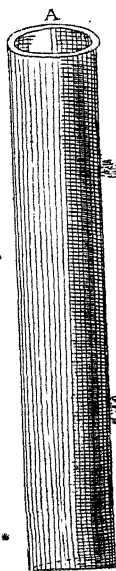


Fig. 5.



Scale of Feet.

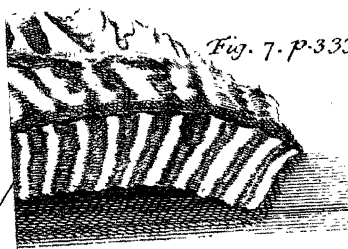


Fig. 7. p. 333.

Hence great Advantages will arise from this Way of Distillation: For, 1. We can keep a greater Fire, if Occasion be, without Fear of breaking our Glasses. 2. The Matter distill'd may be removed as often as we please; whereby we may always prevent the Drawing off any Spirit, &c. too low. And, 3. Any pure, fine, volatile Salt, which shall arise into the second Recipient, will not be so liable to be melted down, either by the Heat, or too watery a Fluid.

IV. *The Case of a Person bit by a Mad Dog : Communicated to the President by Mr. Ranby, F. R. S. Serjeant-Surgeon to His Majesty : From Charles Peters, M. D. F. Coll. Med. Lond.*

Read Jan. 24. 1744-5. **J**OHN Neale, of a robust Constitution, aged Forty-five, had, for some Years, followed the Occupation of curing Dogs: And, on *Thursday* preceding *Michaelmas* Day 1741, being employed in that Calling, he, endeavouring to drench one supposed to be mad, was bit in the Thumb.

The Day following the Dog was observed to droop, refuse his Food, and at Night he died.

The Patient, having been frequently conversant with the like Accidents, was sufficiently alarmed at the Danger; and, having been, the Year before, received a Patient into *St. George's* Hospital, repaired thither for Relief.

Doctor *Hoadly* attending, in the Room of Doctor *Bailey*, the Physician of the Week, directed him to have the Wound scarified, be blooded, use the *Pulvis antilyssus*, and Cold Bath.

About a Fortnight after the Accident, being the Full of the Moon, his Symptoms became so violent, that I was desired to meet my Brethren in Consultation. I found him sitting on a Bed, with one of his Legs tied to the Post; and, upon inquiring how he came placed in that Posture, he told me, He himself fastened the Cord, apprehending he might grow mischievous: And, upon our proposing to him to put on the strapp'd Waistcoat, he readily consented to it; expressing great Dread of becoming hurtful. He told us, He had felt a Numbness in the wounded Thumb, which shot up his Arm to the Shoulder; and that he was not sensible of having taken a Moment's Rest since the Accident had happened; and that, upon endeavouring to compose himself, he fell into Startings, and dreadful Apprehensions of Mischief from Dogs. His Eyes look'd wild, and he complained of an excruciating Pain in the Head. For some Days past he had been troubled with a Difficulty in Swallowing: I proposed to him to get down a small Piece of Bread; but he seemed to refuse it with great Abhorrence: However, being encouraged to make use of his Resolution (which he possessed to an extraordinary Degree), he forced it into his Mouth; where holding it for some Minutes, he endeavoured to swallow, but was seized with violent Spasms, beginning at the Bottom of the *Abdomen*; which, by a convulsive Progression, heav'd itself into the *Thorax*; from whence the Spasms were extended to the *Pomum Adami*;

Adami; when the Patient fell into Strangulation, and afterwards Privation of Sense. Upon recovering from the Fit, as we perceived, that, notwithstanding these Obstacles, he had swallowed the Bread, allowing him Time to rest, we proposed to him to endeavour to swallow a Spoonful of Liquid, which he seemed most shock'd at: He answered with Fierceness, That he could not away with Drink: However, upon Recollection, he said, He would endeavour it: And, taking a Spoonful of Ale-house Drink into his Mouth, he was instantly seized with Convulsions, beginning from the Bottom of the *Abdomen*, and ascending with great Violence to the Head, till he fell into a Fit of longer Duration than the former: However, he swallowed the Liquor; and, upon his recovering his Senses, he pointed with great Vehemence to his Arm, signifying, that he desired to be blooded; from which, he afterwards told us, he had before found Relief.

As his Pulse was extremely hard; we directed the Surgeon to take away 16 Ounces, which proved *ad Deliquium*: However, that soon passing off, his Pulse still continued hard, his Flesh hot, with grievous Complaints of the Pain in his Head.

Upon considering his Case, as he had received no Relief from the ordinary Treatment of this Distemper, and that his Symptoms were now become highly inflammatory (Blood sify, and flammeous Urine); we resolved to lay aside all Thought of Infection, and to betake ourselves to the Method of Cure in inflammatory Fevers; with this Difference only, That, as he had pass'd some Days without Stools, he was directed to take an *Enema*, immediately: and then
the.

the following *Bolus*; *Nitri purif.* 3 ℥, *Confect. Mithridat.* ʒ i, *sexta quaque Hora, addendo Dosi vespertin.* *Extr. Thebaic.* grj. & *repetatur tertia quaque Hora (nisi interveniant Symptomata), donec concilietur Somnus: Epispastica applicentur Brach. & Crurib. intern.*

Upon visiting him in the Morning, the Nurse informed me, That, after having taken two Bolus's, he had slept about half an Hour, to his infinite Refreshment. His Blisters discharged plentifully; his Mind more composed; and his Horrors were so far mitigated, to swallow half a Pint of Ale at one Draught, tho' not without Repugnance. He still complained of living in a Flame; his Eyes ready to start out of his Head, where his Pain still remained acute; not without Numbness in the diseased Arm; Inquietude; Difficulty in Swallowing and Respiration. *Mittatur Sang. e Br. ad ʒ xii. & pergat in Ufu Præscriptorum.*

The Night following he took two Bolus's, and slept near three Hours. The Symptoms appeared less violent the next Day, but still threatened Mischief. *Applicantur Cucurbit. Occipit. ad extract. Sang. ʒ viii. Epispastic. Laterib. Colli, &c.*

His Body was kept soluble with *Manna*, and other cooling Laxatives.

This Method, with little Variation (such as Leeches, *Pulv. sternutatorius, &c.*), was continued for the Space of about fourteen Days, the Blisters being kept open during the whole Time; during which the Symptoms gradually abated. He fell into Languors, which were easily removed by the Use of *Assa fetida, Valerian, &c.*

As he was now free from any Disorders in his Head, and his Pulse beat with a natural Softness, I advised him to return to the Use of the Cold-Bath, *Pulv. Antilyssus*; with a Caution to bleed, and discontinue the Use of them, whenever he found himself heated.

He is now restored to a tolerable State of Health (except at the New and Full Moon): For, tho' he feels some Alteration in the *Quarters*, they are not so considerable; at which time his Symptoms return in some Degree; but so slightly, as not to prevent him from following his Calling, which he has changed to Selling of Greens; not being intirely freed from the Dread of Dogs.

I had forgot to mention, That, during his Illness, he voided so great a Quantity of *Saliva*, that his Teeth, tho' naturally firm, became loose, and continued so, till the Abatement of the Complaint.

As copious Bleeding is generally prescribed for the Cure of this Distemper, I shall make no other Remark upon that Advice, than by observing, That frequent Recourse was had to it, to the apparent Relief of the Patient; who thought himself likewise much benefited by the *Nitre* and *Mithridate*.

As the Difficulty of Swallowing, in the present Disorder, was evidently spasmodic, and infinitely abated by Rest, tho' for half an Hour only, I submit to your better Judgment, how far Opiates may be conducive to the Cure in this Distemper; not only by giving them internally, but likewise by externally rubbing in such a Quantity, as may seem reasonably calculated for the Removal of spasmodic Tensions.

During

During this Illness, he complained of Coldness in the Extremities, with four Eructations; so that, as soon as it was judged safe, he was directed to make use of a Vomit; which was repeated many times with Success.

I observed, when any Liquid was offered him, he pour'd it into his Mouth with uncommon Hastiness: And, upon inquiring his Reason for doing so, he told me, He had experienced, that, by throwing in a large Quantity of Liquid into his Mouth at once, his Faculty of Swallowing became more easy: And that, whenever any Hindrance happened in the Performance, it was not without Difficulty that he recovered himself.

I desire I may no way be thought to depreciate the Efficacy of the *Pulv. Antilyssus* * and Cold-Bath; for I believe them more generally successful than any other Means: Yet I think it clear in the above Case, that they were so far from alleviating the Complaints, that they tended evidently to promote them; the Patient never making use of the Cold-Bath, but his Head-ach increased †, and his feverish Complaints grew more violent.

As the *contra Rabiem* Powder now stands in our *Pharmacopœia*, it is compounded of two Drugs only. I have endeavoured to discover what Effect might be procured by the *Liverwort*; but, upon trying it, for Experiment's sake, in several different Cases, even in large Doses, I could never perceive the least Alteration either in the Pulse, or Secretions.

V.

* Of *Dampier*, as alter'd by Dr. *Mead*.

† Therefore, in my *Thes. Inaug. Lugd. B.* 1724. I propos'd the Use of *warm Baths*; for by them Heat and Thirst will be abated, and the Blood diluted, not render'd still more thick by Sweating, as is the Effect of *Cold Baths*. [See these *Transf.* N^o. 443, p. 319, and p. 360.] C. M.

V. *An Account of a new Species of Fungus,*
by John Martyn, F. R. S. Prof. Bot. Cantab. communicated in a Letter to the President.

Read Jan. 24. 1744-4. **I**N the latter Part of the Summer of the Year 1744, Mr. *Ebret* the Painter brought me a *Fungus* of a very extraordinary Shape and Size, which had been found growing on a Piece of the Trunk of an Elm, in a damp Cellar in the Hay-Market.

The whole Plant was about two Feet in Height; and, at first Sight, seemed not very unlike the Horns of some Deer, being variously branched, and covered with a thick Down. It was of a spongy Substance, and of a dusky-red Colour inclining to Black. The Tips of the smaller Branches were of a Cream-Colour. The larger Branches, or rather the Tops of the whole Plant, were expanded in Form of a Funnel, smooth on the concave, and full of Pores on the convex Side. The inner and lower Part of the Funnel was of the same Colour with the Stalk; the rest of it was of a Cream-Colour.

I have not been able to find, that this Plant has been mentioned by any Author; and am persuaded, that it is a new Species; and, perhaps, the remarkable Branching of the Stalks may induce some to think it a new *Genus*. As the Funnel may be esteemed a Cap, and as this Cap is not lamellated, it will be a *Boletus*, according to the Method observed in the Third Edition of *Ray's Synopsis*. According to
L I *Micheli,*

Micheli, it seems to belong to the *Genus* of *Polyporus*. This Method, which I have long used in the Distribution of this Class, is expressed in the following synoptical Table, which, I think, comprehends all the Species hitherto known.

FUNGI sunt,

lamellati,

- { cauliferi ; AMANITA.
- { sessiles ; AGARICOIDES.

porosi,

- { cauliferi ; BOLETUS.
- { sessiles ; BOLETOIDES.

cancellati, aut scrobiculis excavati ;

- { ex pila crumpentes ; PHALLUS.
- { ex pila non crumpentes ; MERULIUS.

echinati ; ERINACEUS.

in pulverem abeuntes ; LYCOPERDON.

solidi,

- { cauliferi ; CHANTERELLA.
- { sessiles,

- { calyciformes ; PEZIZA.
- { non calyciformes,

{ in longitudinem producti ; DIGITELLUS.

{ horizontaliter prodeuntes ; AGARICUS.

{ subterranei ; TUBER.

According to this Method of mine, as well as that of the Editor of *Ray's Synopsis*, the Plant in Question will be a *Boletus*: And, as I do not think it necessary to constitute a new *Genus*, I have taken the Liberty to call it

BOLETUS caule ramoso ; summitatibus concavis expansis ; ramis minoribus in acutum mucronem desinentibus. Vide TAB. II. Fig. 1.



Fig 1 p 264

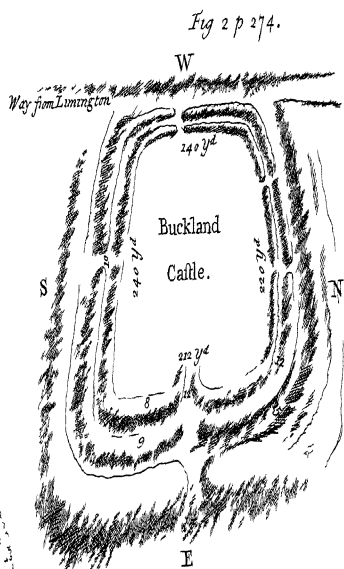


Fig 2 p 274.

Ambrose Farm.

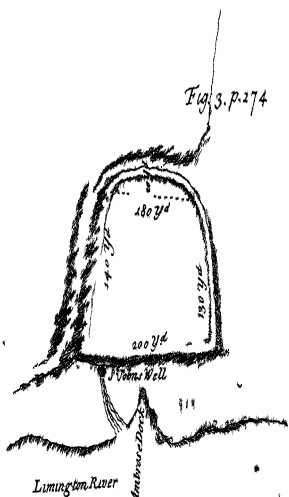


Fig. 3. p. 274

Lumington River

VI. *Extracts of Two Letters from the late Roger Gale, Esquire, F. R. S. to Mr. Peter Collinson, F. R. S. concerning the Vegetation of Melon Seeds 33 Years old; and of a fossil Skeleton of a Man.*

Scruton in Yorkshire, Jan. 14. 1743-4

S I R,

Read Jan. 24. ***
1744-5.

ABOUT this time Twelve-month, I found, accidentally, a Paper of Melon seeds that I had laid by, with the Date of the Year 1710 upon it. I sowed some of them, not with any great Hopes of their coming up; but, to my great Surprize, I had a fine Number of Plants from them, which all prosper'd very well, till they had put out four Leaves, when they were all lost by an Accident. This I have mention'd to you, because, in *Philosophical Transactions*, N^o. 464. Mr. *Triewald* has given an Account of some old Melon-seeds that produced Fruit, tho' they exceeded mine 10 Years in Age: However mine may be a Confirmation of their long Retention of their vegetative Quality; which I suppose may be ascribed to the Oilyness of the Seed, and the Hardness of its outward Coat.

We have few or no Fossils in this Country; but a Friend in *Staffordshire* [Mr. *Platt*] informs me, that That Country abounds much in Fossils; such as Sea-Shells, Rock-Plants, and other marine Bodies left at the Deluge. Near *Bakerwell* in *Derbyshire* was lately found the Skeleton of a Man, with some Stags Horns, in digging a Lead-Mine.

[In the second Letter, dated *Scruton, May 19. 1744.* Mr. *Gale* gives the following Account of this *Skeleton* in Mr. *Platt's* own Words, from a Letter written to himself by that Gentleman.]

Dear Sir,

THE Skeleton I formerly mentioned to you was found at *Lathill-dale*, near *Tolgrave* and *Bake-well* in *Derbyshire*, as the Workmen were driving a Sough, or Drain to a Lead-Mine, about nine Yards deep from the Surface of the Earth, and about forty Fathom from the Beginning of the Sough. There were found with the Skeleton Stags Horns; two Pieces of which I have now in my Custody; *viz.* the Brow-Antler, which is nine Inches long, and seems to have about two Inches broke off the Tip-End; the other is a Piece of the large Horn near the Head, and is three Inches Diameter. Both the Horns of the Stag, and the Rib-Bones of the Skeleton, are much decayed; and as soon as the Head of the latter was exposed to the Air it crumbled all away, except a Piece of the lower Jaw; now also so imperfect as not easily to be distinguished what it has been. Several of the larger Teeth were taken out, which were covered with their natural Enamel, and perfectly found. The Place where these Things were found, is on every Side surrounded with a rocky petrified Substance, or *Terra lapidea*, by the Miners called *Tuft*, so hard (as they say) as to strike Fire against their Tools. This Substance lay above the Bones and Horns a Yard and half thick or more, and on either Side; and beneath them to a Breadth and
Depth

Depth uncertain: So that it appears, that the Skeleton and Horns lay in a Cavity, which was not however contiguous to them, there being a sort of soft coarse Clay or Marl interspersed thick with little petrify'd Balls, or Pellets of the same kind of Substance as the Tuft, for near a Quarter of a Yard round them; but none of the Bones seem'd in any Degree to be petrify'd. The Workmen conjectur'd there was more of the Skeleton to be found; but they dug no further than was necessary to complete their Sough.

This, dear Sir, I believe, is, with what I sent you before, as full and accurate an Account, as you will obtain from any other Hand, of this odd Discovery. The Interment of this Man and Stag seem to me to have been accidental, by their falling into a Chasm or wide Cleft of the Rock in very early Times; which has since closed up, and grown over them, by the Accretion of the marly Substance, which environs the Skeleton, &c.; and in time, perhaps, will grow as hard as the Tuft, and rest of the Rock. If you have any other Particulars come to yourself, or the *Royal Society*, I should be glad to know them. I have desired Mr. *Platt* to send you up a few of the Pellets, with the Fossils he designs you, in a short time; and am

*Your most obliged Friend,
and humble Servant,*

R. Gale.

By covering up my Trees with Ivy, in *February* I have vast Quantities of Apricots and Peaches, while my Neighbours have hardly any.

VII. *A Letter from W. Watson, F. R. S. to Francis Wollaston, Esquire, F. R. S. concerning a large Stone found in the Stomach of a Horse.*

S I R,

Read Jan. 31. 1744. **I** Hereby do myself the Pleasure of sending you a few Observations upon the *Calculus* you favour'd me with the Examination of. You inform'd me, that it was found in the Stomach of a Coach-Horse in *July* last; and that it then weigh'd 3 Pounds two Ounces and half *Averdupois*. You likewise mentioned, that the poor Creature was observed frequently to be in violent Pain; and would sometimes eagerly turn his Head to one of his Sides, and sometimes to the other, as though he endeavoured to bite out that which annoyed him; and that he died, after having taken various Remedies, which the Farriers administer'd. When I weighed this Stone about ten Days ago, its Weight was 2 Pounds and 3 Quarters of an Ounce; so that, in about half a Year, it had lost 1 Pound 1 Ounce and 7 Eighths. The Figure of it is spheroidal, as these Sort of Stones generally are; its Periphery 17 Inches and $\frac{1}{4}$, by 16 Inches and half; which are very near the same Dimensions this Stone had when first found. The Surface of it irregular, somewhat resembling the Inequalities observed upon the Surface of the Brain; all the projecting Parts of which are polished, from their Friction against the Sides of the Stomach. It is of a dark-brown bilious Colour, and very like to a
Species

Species of *Pyrites*; insomuch that, unless it were taken in the Hand, whereby their specific Gravities may be determin'd, it might pass for one of that Family; altho' this Stone is by much the most specifically heavy I ever saw of this sort.

I imagine, that, a considerable Time before the Horse died, by some Accident this Stone received a great Blow; for there appears to have been a Piece broken out; and there are two large Cracks not yet fill'd up; near which terminate several concentric Circles. This Stone seems to involve a smaller one, altho' no-where perfectly separated from it; but the outward is by much the hardest. In the Centre are two Holes, in which may be seen several Hairs of the Horse; but I have not been able to find any other extraneous Body, upon which these *Calculi* are usually formed.

Having, from sawing the Stone, a Quantity of its Powder, I was induc'd to an Inquiry into its constituent Parts by way of Analysis.

I first let fall two small Pieces of this Stone into Water almost boiling: They immediately sunk, but arose again, and continued alternately rising and sinking a considerable time. This was occasioned by the Quantity of Air-Bubbles, which the Heat rarefy'd; but the Air was detain'd by the *Mucus*, which seem'd to connect the Particles of the Stone together; and which, tho' dilut'd by the hot Water, was tenacious enough to form Bubbles of Size sufficient to buoy up the Pieces of Stone; the Rarefaction growing greater, the Bubbles burst, and the Stone fell to the Bottom; but arose again, in like manner, at the Expulsion of more Air. The learned Dr. *Hales* likewise

wife found great Quantities of Air in the human *Calculus*.

I infused two Drams of this Powder in two Ounces of boiling Water: This Infusion I filtered when cold. It was of a light-brown Colour, and of a bitterish saline Taste. I calcined what remained of the Powder after the Infusion, till the Whole was black, and then it weighed a Dram and three Grains. I made the following Trials with the Infusion.

1. Mixed with Syrup of Violets, it became green.
2. With Oil of Tartar, the Colour was deeper without Ebullition, but the Mixture sent forth immediately a strong urinous Smell; the same Smell arose from rubbing some of the Powder with Oil of Tartar.
3. With Oil of Vitriol, and Spirit of Salt, it lost its Colour; but no Ebullition ensued.
4. With a Solution of Sublimate in Water, the Mixture curdled, and let fall a light-grey Sediment, leaving the Liquor quite transparent.
5. With a Solution of Sublimate in Lime-Water, the Mixture grew turbid, and let fall a deep-yellow Sediment, in a much greater Quantity, and of a deeper Colour, than a Solution of Sublimate and Lime-Water alone.

From these Inquiries it appears, that the Stone is compounded of an Earth, Air, *Mucus* of the Stomach, and a saline Principle bearing great Resemblance to *Sal Ammoniac*.

If these Hints meet with your Approbation, I shall think it a sufficient Recompence for the Trouble I have taken; and am, with great Respect,

S I R,

Your most Obedient,

Aldersgate-Street, Jan. 26.

1744.

Humble Servant,

W. Watſon.

VIII. *Extract of a Letter from Bombay, dated January 23. 1743-4. communicated by Francis Woolaſton, Eſquire, F. R. S. of a Porcupine ſwallowed by a Snake.*

Read Jan. 31. **S**OME time ago there was found, on 1744. an Iſland adjacent to this, a large Snake, dead, with a Porcupine in its Belly. Perhaps you may not know, that a Snake always ſucks in its Food by degrees. This had ſeized the Porcupine by the Head; and had ſo ſucked it in. When it was quite in, the Quills, which were flattened down whilſt it was going in, roſe; ran through the Snake's Belly; and killed it: So that there was a monſtrous large Snake dead, with the Quills of a Porcupine ſticking out of it in many Places.

IX. *Extract of a Letter from Dr. Josephus Laurentius Bruni, of Turin, F. R. S. to Mr. Henry Baker, F.R.S. concerning the Bologna Bottles.*

Dated at Turin, Nov. $\frac{22}{11}$ 1744.

Dear Friend,

Read Jan. 31. 1744-5. **T**HE Seed of the *Gramen tremulum**, which you favoured me with, has amazed all here who have seen it. The curious People in this Country talk much of a Phenomenon, which is called of the *Bologna* Bottle, because it was first discover'd at *Bologna*. If you let these Bottles fall perpendicularly from some Height upon a Brick-Floor, they will not be broken; but if you drop into them some little hard Bodies, they will burst in Pieces. I will give you an Account of what I try'd myself about them.

I took one of these glass Bottles, whose Form resembles a *Florence* Flask, and whose Capacity is about three Quarters of a Pint, and let it fall down from the Height of five Feet and half upon a Floor of Brick, and it was not broken: I then let fall down into it, from the Mouth to the Bottom internally, a Piece of Flint-Stone, weighing 11 Grains; and immediately the Bottle burst into many Pieces.

I took one of those Pieces, weighing a Dram, and let it fall in the same manner into another Bottle, which I moved circularly for a Minute; and then
putting

putting it upon a Table, in about a Quarter of an Hour it broke in Pieces.

Into a third Bottle I dropped a Piece of Whetstone, weighing 40 Grains; and in some few Minutes the Bottle was broken.

I filled another Bottle half-full of Water, and let fall into it a small Piece of Flint stone; and after four Hours it burst.

I let fall into three other Bottles a Piece of Wood weighing 50 Grains, a Piece of Brass weighing 300 Grains, and a Ball of Lead weighing 140 Grains; and neither of them was broken.

These Bottles are thicker at the Bottom than the Neck. The Glass-Maker blows them, and lets them cool, without putting them again into the Oven. And, from the Experiments, I take notice, that what is capable of breaking them ought to have some Roughness: And I am told that a Grain of River-Sand will break them.

The first Opportunity I shall send you two or more of these Bottles; and am, &c.

X. *A Letter from Mr. Tho. Wright to James Theobald, Esq; F. R. S. concerning Two ancient Camps in Hampshire.*

Read Feb. 7. 1744 5. **L**AST Summer, during my Stay in the West of *England*, common Report, and my own natural Curiosity, led me to a Place in *Hampshire* called *Buckland Castle*, or, more vulgarly, the *Rings*; where I found two neighbouring
M m 2 Camps

Camps about three Furlongs asunder: The one very strong, with double Ditches, and triple *Vallums*, upon the Top of an Hill, three Ways guarded by a natural Ascent; the other upon a lower Ground, close by a River (which defends it on one Side), with a Ditch and *Vallum* half round, and a kind of Morass on the other. The first contains about ten Acres; the latter about seven; and the Land lying betwixt them is, and has been time out of Mind, called *Ambrose Farm*: Besides an Arm of the River, or rather of the Sea (it being Salt-Water), running close up to the latter, is called *Ambrose Dock*.

From all which it appears not improbable, that this may have been a principal Station belonging to *Aurelius Ambrosius*.

The inclos'd is a true Representation both of their Form and Situation (*see* TAB. II. *Fig.* 2, 3.): The large one is about a Mile from *Limington*; and, cross the River opposite to the Town, and facing the South-east Angle of it, is an artificial Hill, known by the Name of *Windmill Nap*; which I take to have been a Beacon, by reason it commands almost all the *Isle of Wight*, the full Passage of the *Needles*, and great Part of the there level Champagne, besides the Camp itself.

Camden (tho' he takes no Notice of these Camps, yet mentions one of much less Consequence, about twelve Miles Distance, called *Castle-Malwood*) says, "It is most certain, that, about the Year 508, *Aurelius Ambrosius* had here many Conflicts with the *Saxons*, with various Success." But, again, the People of this Country have a Tradition, That, three Miles to the West of this Camp, a famous Battle

was fought betwixt the *Saxons* and the *Danes*; in which so much Blood was spilt, that a little River is said to have run Blood, now called from thence *Danes Stream*.

Your Friend, and

*Oxenden-Street, Jan. 12.
1744.*

Humble Servant,

Tho. Wright.

XI. *Observations on a Case published in the last Volume of the Medical Essays, &c. of Recovering a Man Dead in Appearance, by distending the Lungs with Air. Printed at Edinburgh, 1744; by John Fothergill, Licent. Coll. Med. Lond.*

Read Feb. 21. 1744-5. Now printed with Additions. **T**HERE are some Facts, which, in themselves, are of so great Importance to Mankind, or which may lead to such useful Discoveries, that it would seem to be the Duty of every one, under whose Notice they fall, to render them as extensively public as it is possible.

The Case which gives Rise to the following Remarks, I apprehend, is of this Nature: It is an Account of "*A Man, dead in Appearance, recovered by distending the Lungs with Air; by Mr. William Tossack, Surgeon in Alloa;*" printed in Part 2. p. 605. Vol. V. of the *Medical Essays*, published by a Society of Gentlemen at *Edinburgh*; an Abstract of which will

will be sufficient in this Place: Those who desire an ampler Account may consult the Article itself.

A Person suffocated by the *nauseous Steam* arising from Coals set on Fire in the Pit, fell down as dead; he lay in the Pit *between half an Hour and three Quarters*; and was then dragg'd up; *his Eyes staring and open, his Mouth gaping wide, his Skin cold; not the least Pulse in either Heart or Arteries, and not the least Breathing to be observed.*

In these Circumstances, the Surgeon, who relates the Affair, *applied his Mouth close to the Patient's, and, by blowing strongly, holding the Nostrils at the same time, raised his Chest fully by his Breath.* The Surgeon immediately felt six or seven very quick *Beats of the Heart; the Thorax continued to play, and the Pulse was soon after felt in the Arteries.* He then opened a Vein in his Arm; which, after giving a small Jet, sent out the Blood in Drops only for a Quarter of an Hour, and then he bled freely. In the mean time he caused him to be pull'd, push'd, and rubb'd, as much as he could. In one Hour the Patient began to come to himself; within four Hours he walked home; and in as many Days returned to his Work.

There were many Hundred People, some of them of Distinction, present at the Time.

This is the Substance of the Account; from whence it naturally appears how much ought to be attributed to the Sagacity of the Surgeon in the Recovery of this Person. Anatomists, it is true, have long known, that an artificial Inflation of the Lungs of a dead or dying Animal will put the Heart in Motion, and continue it so for some time; yet this is the first Instance

Instance I remember to have met with, wherein the Experiment was applied to the happy Purpose of rescuing Life from such imminent Danger.

Bleeding has hitherto been almost the only Refuge upon these Occasions: If this did not succeed, the Patient was given up. By Bleeding, it was proposed to give Vent to the stagnating Blood in the Vein, in order to make Way for that in the Arteries *à tergo*, that the Resistance of the Heart being thus diminished, this Muscle might again be put in Motion.

But, in too many Instances, we every Day are informed, that this Operation will not succeed, tho' the Aperture is made with never so much Skill: Nor is it likely, that it should, when the Blood has lost considerably of its Fluidity, the Motion of the Heart, and the contractile Force of the Solids, are at an End.

Chasing, Rubbing, Pulling, the Application of Stimulants, are too often as ineffectual as Bleeding.

The Method of distending the Lungs of Persons, dead in Appearance, having been try'd with such Success in one Instance, gives just Reason to expect, that it may be useful to others.

It may be a proper Inquiry, In what Cases, and under what Circumstances, there may be a Prospect of applying it with Success?

It will at once be granted, That when the Juices are corrupted, where they are rendered unfit for Circulation by Diseases, where they are exhausted, or where the Tone and Texture of the Solids is injured or destroyed, it would be extreme Folly to think of any Expedient to recover Life.

But where the Solids are whole, and their Tone unimpair'd by Diseases, the Juices not vitiated by
any

any other Cause than a short Stagnation; where there is the least Remains of animal Heat, it would seem wrong not to attempt so easy an Experiment.

This Description takes in a few Diseases, but a greater Number of Accidents. Amongst the first are many of those which are called sudden Deaths from some invisible Cause; Apoplexies, Fits of various Kinds, as Hysterics, *Syncopes*, and many other Disorders, wherein, without any obvious Præ-indisposition, Persons in a Moment sink down and expire. In many of these Cases it might be of Use to apply this Method; yet without neglecting any of those other Helps, which are usually called in upon these melancholy Occasions.

It is not easy to enumerate all the various Casualties, in which this Method might be try'd not without a Prospect of Success; some of them are the following: Suffocations from the sulphureous Damps of Mines, Coal-pits, &c. the condensed Air of long-unopen'd Wells, or other subterraneous Caverns; the noxious Vapours arising from fermenting Liquors received from a narrow Vent; the Steam of burning Charcoal; sulphureous mineral Acids; arsenical *Effluvia*, &c.

Perhaps those, who, to Appearance, are struck dead by Lightning, or any violent Agitation of the Passions, as Joy, Fear, Surprise, &c. might frequently be recover'd by this simple Process of strongly blowing into the Lungs, and by that means once more communicating Motion to the vital Organs.

Malefactors executed at the Gallows would afford Opportunities of discovering how far this Method might be successful in relieving such as may have
unhappily

unhappily become their own Executioners, by hanging themselves. It might at least be try'd, if, after the Criminals have hung the usual Time, inflating the Lungs, in the Manner propos'd, would not, sometimes, bring them to Life. The only ill Consequence that could accrue from a Discovery of this kind would be easily obviated by prolonging the present allotted Time of Suspension.

But this Method would seem to promise very much in assisting those who have been suffocated in the Water, under the above mentioned Circumstances; at least it appears necessary to recommend a Trial of it, after the Body has been discharged of the Water admitted into it, by placing it in a proper Position, the Head downwards, prone, and, if it can be, across a Barrel, Hoghead, or some such-like convex Support, with the utmost Expedition.

It does not seem absurd, to compare the animal Machine to a Clock; let the Wheels whereof be in never so good Order, the Mechanism complete in every Part, and wound up to the full Pitch, yet, without some Impulse communicated to the *Pendulum*, the Whole continues motionless.

Thus, in the Accidents described, the Solids are supposed to be whole and elastic, the Juices in sufficient Quantities, their Qualities no otherwise vitiated than by a short Stagnation, from the Quiescence of that *moving Something* which enables Matter in animated Bodies to overcome the Resistance of the *Medium* it acts in.

Inflating the Lungs, and, by this means, communicating Motion to the Heart, like giving the first Vibration to a *Pendulum*, may possibly, in many Cases,

Cases, enable this *Something* to resume the Government of the Fabric, and actuate its Organs afresh, till another unavoidable Necessity puts a Stop to it intirely.

It has been suggested to me by some of my Acquaintance, that a Pair of Bellows might possibly be applied with more Advantage in these Cases, than the Blast of a Man's Mouth; but, if any Person can be got to try the charitable Experiment by blowing, it would seem preferable to the other: *1st*. As the Bellows may not be at hand: *2^{dly}*, As the Lungs of one Man may bear, without Injury, as great a Force as those of another Man can exert; which by the Bellows cannot always be determin'd: *3^{dly}*, The Warmth and Moisture of the Breath would be more likely to promote the Circulation, than the chilling Air forced out of a Pair of Bellows.

To conclude, as I apprehend, the Method above described may conduce to the saving a great many Lives, as it is practicable by every one who happens to be present at the Accident, without Loss of Time, without Expence, with little Trouble, and less Skill, and as it is, perhaps, the only Expedient of which it can be justly said, that it may possibly do *great Good*, but cannot do Harm, I thought it of so much Consequence to the Public, as to deserve to be recommended in this Manner to your Notice: For tho' it is already published in a Work which is generally read by the Faculty; yet, perhaps, it may be overlooked by some, forgot by others, and perhaps, after all the Care that can be taken, it may never come

to the Knowledge of a Tenth of those who ought not to be ignorant of it.

John Fothergill.

P. S. As the Representation of an extraordinary Fact may perhaps induce some to try the Experiment, when Occasions like those which are specified in the above Remarks occur, it is hoped, that Humanity will prompt all such to favour the Public with an Account of their Success, with the principal Circumstances that attended. And as the Writer of these Remarks has embarked in the Design of rendering this Fact diffusively known, he would be glad to have it in his Power to inform the Public, that numerous Experiments confirm what this Case suggests; *viz.* the Possibility of saving a great many Lives, without risking any thing.

*White-Hart Court, Gracious-Street,
7ber, 1744.*

XII. *A Copy of a Letter from the Reverend Mr. Richmond, to ——— Leigh, Esq; of Adlington in the County of Chester, concerning a moving Moss in the Neighbourhood of Church-Town in Lancashire: Communicated by Edward Milward, M. D. F. R. S.*

Dear Sir,

*Read Feb. 28.
1744-5.*

AS you will probably hear, that this Neighbourhood is greatly alarmed with what they call a Miracle, it may not be unacceptable if I give you the History of it.

On *Saturday* the 26th of *Jan.* 1744-5, a Part of *Pilling Moss*, lying between *Hescomb Houses* and an Estate of *Mr. Buttler's*, called *Wild Bear*, was observed to rise to a surprising Height: After a short Time it sunk as much below the Level, and moved slowly towards the South Side: In half an Hour's time it cover'd 20 Acres of Land. The improved Land adjoining that Part of the *Moss* which moves is a concave Circle containing near 100 Acres, which is well nigh fill'd up with Moss and Water. In some Parts it is thought to be five Yards deep. A Family is driven out of their dwelling House, which is quite surrounded, and the Fabric tumbling down. *Mr. Buttler, Whitehead, and Stephen White*, are the first Sufferers by this uncommon Accident. An intense Frost retards the Regress of the *Moss* To day; but I fear it will yet spoil a great deal of Land. The Part of the *Moss* which is sunk like the Bed of a River,
runs

runs North and South; is above a Mile in Length, and near half a Mile in Breadth; so that I apprehend there will be a continual Current to the South. A Man was going over the *Moss* when it began to move: As he was going Eastward, he perceiv'd, to his great Astonishment, that the Ground under his Feet moved Southward. He turn'd back speedily, and had the good Fortune to escape being swallowed up. I have been at the *Moss* to make Observations every Day this Week: If any thing happens worth your Knowledge, you may depend upon hearing further from,

S I R,

Your very affectionate

Humble Servant,

L. Richmond.

XIII. *A brief Inquiry, by John Ward, F.R.S. & Rhet. Prof. Gresh. into the Reading of two Dates in Arabian Figures, cut upon Stones which were found in Ireland; communicated to the Royal Society, on November 10. 1743. and December 6. 1744.*

Read Feb. 28. 1744-5. **T**WO Dates in *Arabian Figures*, transmitted from *Ireland*, were some time since laid before this Learned Society. But as
the

the Reading, which had been given them, seemed doubtful to the Gentlemen then present, they were pleased to honour me with desiring my further Thoughts concerning them. Both those Dates are said to be cut on Stones, and in Relief. I considered them as carefully, as I was able, and after the strictest Examination could see no sufficient Reason to think either of them so old, as had been represented. For which Opinion I now take Leave to offer the following Reasons.

I. One of the Stones, which was found in the Friars Abbey, and is now fixed in the Wall of a Garden belonging to Alderman *Baldwin* in *Corke*, contains together with the Date several Words cut in capital Letters, as also two human Images (one a Trumpeter and the other a Drummer) with other Ornaments and Decorations. (See TAB. I Fig. 2.) The Date, as there expressed, runs thus: *A. D. 158*. But it is evident, here must be some Mistake; since it is generally allowed by Chronologers, that this Way of computing Time from the Birth of *Christ*, which is called the *Christian Era*, was not introduced till the sixth Century, in the Reign of the Emperor *Justinian*, and is commonly ascribed to *Dionysius Exiguus* (a). It has therefore been supposed, that the Figure 1 is omitted in the Place of Thousands, which would make the Date 1158; and for which there seems to be sufficient Place, as there is not for any other Figure afterwards. But that the Sculpture on this Stone cannot

(a) See Petav. *De doctrin. temp.* L. XII. c. 2, 3. Bever. *Instit. chronol.* L. II. c. 10. Strauch. *Breviar. chron.* L. IV. c. 40. quæst. 4.

cannot be so antient, appears to me highly probable from several Considerations, taken from the Shape of the Figures, Form of the Letters, Spelling of the Words, and Dress of the Images.

As to the Figures, I have never met with the Five any thing like the Shape of it upon this Stone (which comes pretty near the modern Form) till the fourteenth Century; except in one single Instance of a Date 1295, (a) which I had the Honour to communicate to this Society upon the Seventh of June last. In the *Table of Characters* prefixed to N^o. 439 of the *Philosophical Transactions* the Figure Five is given from three Writers of the thirteenth Century, in two Forms both very different from that upon this Stone. One was taken from *Maximus Planudes*, a Greek Writer, which is like the β of that Language inverted in this manner β ; and the other from *Johannes de Sacro Bosco* and *Roger Bacon*, which is made thus q. The latter of these continued in Use till the Beginning of the sixteenth Century, as appears from a Manuscript preserved in the *Inner Temple* (b), bearing Date the *xxi Yere of King Henry the VII. and the Yere of our Lorde 1504*; to which is prefixed a *Calender*, wherein all the Figures are like those of *Roger Bacon*.

The Letters in this Sculpture are mixed, being partly *Roman*, and partly *Saxon*; as we often find, that the Workmen took great Liberties in varying and mixing their Letters. Those of the latter Sort
are

(a) See *Phil. Trans* N^o 474. p. 91.

(b) This they call their *Grace Book*, because it contains, among many other Things, Graces to be used before and after Meals.

are D, G, N, and O, the Shape of which may be seen in the Draught of this Sculpture. The first of them occurs in several Coins of King *Henry* the Eighth, King *Edward* the Sixth, and Queen *Mary*. And the three last are found together, cut in Relief over the Eastern Gate of *Lincoln's Inn*, in the following Date, ANNO DNI 1518, the Form both of the Letters and Figures agreeing likewise with those of the Sculpture. (See TAB. I. Fig. 3.) I shall only add, what our celebrated Antiquary, Mr. *Camden*, has observed, that the *Saxon* Characters were used in *Ireland* in his Time (a). Nothing therefore appears in the Letters, which can necessarily carry the Antiquity of them higher than the sixteenth Century.

Nor is there any thing in the Spelling of the Words, but what agrees with the Manner of Writing in that Century, more especially while the Orthography of the *English* Language continued so various and uncertain, as it did for the greatest Part of it. The only Words, that call for any Remarks are *fere* for *fear*, or *feare* with *e* final, *dow* for *do* or *doe*, and *shall* and *well* with a double *ll*. As to the first of these, we then often find the *a* omitted in Words of that Form; as *clene*, *clere* *clerely*, *nere*, *yere* *yerely*, and the like. And as to *dow* for *do* or *doe*, I meet with *dowthe* for *doeth* or *doth*, and *gowlde* for *golde* or *gold* (b). And such Monosyllables, as *shall* and
well,

(a) *Britann.* pag. 730, edit. 1607.

(b) See Sir *Richard Gresham's* Petition to King *Henry VIII.* Cotton Libr. Cleop. E. 4. Sir *Tho. Gresham's* Memorial to Queen *Mary.* Ibid. Orho, E. X. 3. His Memorial to Queen *Elizabeth* in 1558, found among Lord *Burgley's* Papers, and now in the Hands of *James West Esquire.*

well, were in the former Part of that Century more commonly written with a single *l* than double *ll*, but afterwards promiscuously, as may be seen by the Bibles printed in those Times.

With regard to the Images, King *Philip* is the first, whom I have seen dressed with a high crowned Hat and Feather. Nor does it seem improbable, that he might introduce that Fashion here in *England*. Ruffs do not appear among us till pretty late in the Reign of King *Henry* the Eighth, and were small at first; but afterwards they increased in their Size gradually, till they became very large under Queen *Elizabeth*, and so continued thro' the next Reign, and Part of the following, when they were succeeded by broad laced Bands. It is true indeed, that we find Ruffs upon the Images of some of our Princes, or other great Persons, placed on their funeral Monuments, and elsewhere, which seem to exceed the Fashion then in Use. But as these Images were made after their Death, so the Habits are suited not to their Times, but those when the Artists lived, and the Monuments were erected, where they are found; which Circumstance, if not attended to, will be apt to mislead us in several other Things relating to our Antiquities. The close striped Jackets, shaped to the Body, with small Buttons, and striped Breeches, came in about the same time with the Ruffs. The ingenious Artist, and diligent Searcher into our *English* Antiquities, Mr. *George Vertue*, shewed me a Portrait of *William Herbert*, the first Earl of *Pembroke*, so habited in the Reign of King *Edward* the Sixth; which, he said, was the oldest Picture, that he remembered to have seen in that Habit. And he had likewise
 O o another

another of *Edward Clinton*, Earl of *Lincoln*, and Lord Admiral in Queen *Elizabeth's* Reign, dressed with a Cap and Feather, a striped Jacket, small Buttons, and a Ruff.

These several Considerations induce me to think, that this Sculpture was made in the sixteenth Century, and probably not before the Reign of Queen *Elizabeth*, considering the Largeness of the Ruffs, and some other Circumstances already mentioned, which seem not to suit with more early Times. And from thence I would conclude, that the Figure omitted in the Date must have been a Decimal. Some Figure is evidently wanting, which, had it been a 1 in the Place of Thousands, might easily have been supplied; tho the Sculpture is in Relief, by fixing in a small Piece of Stone in the Form of that Figure. And that this Mistake was owing to the Workman may be presumed from hence, that the Word *you* is likewise omitted in the Sentence below.

II. The other Stone was found in an old Castle since pulled down, which was called *Kilbritain*, in *Irrish* the *Church* or *Cell*, or rather the *Burial Place of the Britains*. It is now at the House of Mr. *Stowel* at *Kilbritain* near *Corke*, and contains some imperfect Remains of an Inscription, besides the Date. (See TAB. I. Fig. 4.) Two Copies of it have been transmitted hither, one of which being taken by laying a clean Sheet of white Paper over the Stone, and tracing out the whole Inscription with a Blacklead Pencil, must therefore be the more exact. But so small a Fragment only of the Stone has been preserved, that no certain Judgement can thence be made of the Whole; and not one perfect Word remains

mains upon it, unless perhaps the last. And as to the Date, which has been read 1035, I cannot come into that Sentiment for these Reasons.

The same Objection lies here against the Shape of the Figure Five, as in the other Date, for the Reason there given. And what has been taken for a Cipher in the Place of Hundreds, I apprehend to be only an imperfect Four, which wants the oblique Stroke, descending on each Side transversly from the circular Part in this manner Q; for which there appears to be Room at the Bottom of the Circle, which does not come so low as the other Figures, before and after it, tho it equals them in Hight at the Top. There is a Sample of this Figure both in *Johannes de Sacro Bosco* and *Roger Bacon*, as also in the Manuscript of the *Inner Temple* cited above, which shews that it remained in Use till the sixteenth Century. As this Inscription is cut in Relief, the extreme Parts of that Figure might as well be broken off, as the much greater Parts of the mutilated Letters in the Lines above it. And I would further observe, that the Probability of this Opinion seems to be not a little confirmed by a parallel Instance of the same Figure, which formerly came before this Society, in a Date found at *Colchester*; the Figures of which were at first thought to express the Year 1090, that in the Place of Hundreds being taken for a Cipher, as in the present Case, by not attending to the side Strokes, which were pretty near defaced; till upon a more accurate View of the Original by a very worthy Member, and skilful Antiquary, that Mistake was discovered, and the Date found to be 1490. For a further Account of which, together with a Draught

of the Date, I must beg Leave to refer to N°. 439 of the *Transactions* mentioned above. But as this Manner of writing the Four appears as antient as the Time of *Johannes de Sacro Bosco*, who died in the Year 1256, the Age of this *Irish* Date cannot so well be determined by that Figure, as by the Form of the Five, which follows it, and so exactly agrees with the modern Shape, that it cannot, I think, from any Instance I have yet met with, be justly deemed much older than the sixteenth Century.

Upon the best Judgment therefore, which I can form of these two Dates, from such Arguments as the Nature of the Subject admits of, it appears to me highly probable, that neither of them can be more antient, than the Times I have here assigned them.

Gresham College,
Feb. 21.
1744-5.

J. Ward.

XIV. *A Letter from the Reverend Henry Miles, D. D. F. R. S. to Mr. Henry Baker, F. R. S. of firing Phosphorus by Electricity.*

Dear Sir,

Read March 7. 1744-5. **I**T came into my Head last Night, to try whether the *Effluvia* of an excited glass Tube would not kindle *Phosphorus*; and, having been using my Tube for the sake of a little Exercise, I took a small Bit of about a Quarter of an Inch

Inch long, which has lain by me these ten Years; and having nothing at hand convenient for holding it, I roll'd it up in a small Piece of white Paper; and applying it to the excited Tube, it immediately took Fire, emitting a considerable Quantity of Flame and Smoke: After some time I quench'd it, by dipping it into Water, which was ready for that Purpose; and taking it out again without staying any longer than to be satisfied it was not on Fire, I applied it as before, when it suddenly took Fire, as at first: This I repeated in the same manner for six or seven times with the like Effect; tho' the *Phosphorus* could not be drained of the Water, especially as the Paper about it was wet.

The Room in which I made the Trial was not absolutely dark, having a dull Fire (tho' without any Candle): The Tube I use is about two Feet and a half long, the Diameter of the Bore nearly one Inch, the Thickness about one Eighth of an Inch, hermetically sealed at one End (which Sort are, by the way, most convenient for rubbing): The *Phosphorus* was held generally about five Inches from the Tube; but once or twice bringing it nearer, I could perceive a continued Ray of Light from the Tube to the *Phosphorus*. Some Occasions calling me away in the midst, I could not be more accurate; but I would not omit to tell you one Observation I made, upon pretty smartly exciting the Tube, that the Coruscations of Light were larger, more substantial, and of a more regular Form than I had ever observed them before, This happen'd, not when the *Phosphorus* was applied, but in the Intervals. Whether any of the Fumes of the *Phosphorus*, which remained in
the

the Room, might contribute hereto, I cannot tell, tho' it is not very likely. I shall attempt below to give you, as well as I can, the Form and Size of the Rays of Light, as they appeared immediately after my withdrawing my Hand, without applying my Finger, as is usually done to produce the Snapping of the *Effluvia*. Tho' I never made many Trials with *Phosphorus*, yet as I am not insensible, that some solid Kinds of it will be inflamed by the mere Action of the Air upon it, when it is taken out of the Water in which it is usually kept; I was therefore minded to try whether the Air would have that Effect upon mine, and accordingly took it out of the Water, with a *Forceps*, and laid it down on a Shelf, so as nothing touch'd it but the Instrument which held it, but I could not perceive the least Glimmering of Light, tho' the Place was sufficiently dark, after it had lain there for the Space of half an Hour, which I thought long enough to satisfy me, that it was not kindled by the Action of the Air upon it in the above-mentioned Experiment.

If you have known this Trial to have been made by any one before, you will cast this into the Fire; and however believe, that I am, with very great Sincerity,

Dear Sir,

Your most affectionate,

and obliged humble Servant,

H. Miles.

See

See TAB. I. *Fig. 5.*

A. Represents the Tube which I held in my Right-hand, and excited with my Left, having on a Glove, which I find more convenient for me in rubbing it. I should observe, that my Method then was to rub it smartly for about half a score times up and down; and then giving it one brisk Stroke, beginning at the End from me, upon discharging my Hand quick from the Tube, the Coruscations of Light appear'd as mark'd α and β , both in Size and Form: Some Allowance may be thought reasonable to be made for one's Judgment in such a Case, the Motion being so very sudden, and the *Phænomenon* so soon disappearing. But I intend to repeat the Experiment whenever the Temperature of the Air shall be favourable, which I don't find it to be this Morning. I forgot to mention, that, during this Trial, I found the *Effluvia* troublesome to my Eyes to a great Degree, occasioning a very sensible smarting Pain, which did not go off for some time; tho' I never designedly brought the Tube near my Face. This was the first time of using this Tube.

XV. *An Observation of a Fracture of the Os Humeri by the Power of the Muscles only; by the late Claudius Amyand, Esq; Serjeant Surgeon to His Majesty.*

Read March 7. 1744-5. **T**HE outward Causes of Fractures not being sufficient to break or tear asunder the broken Pieces of a fractur'd Knee-pan, in

in the Manner we see them in the Fracture of that Bone, the Cause of it hath justly been ascribed to the Power and Action of the Muscles upon it in a morbid State of that Bone, which disposing it to a Fracture when that Power is strongly applied, and affecting both Knee-pans in a like manner, soon brings about a Fracture also of the opposite Bone, then appearing as thoroughly sound as the fractured Part before was thought to have been. Many Instances might be given of the Fracture of the *Patella* in Persons thoroughly sound, where the Part itself had received no Hurt whatever : Which seems to imply, that the Cause of it was to be ascribed to the Muscles only; and the rather, that many Instances may be given of the Fracture in the Neck of the *Os Femoris*, in the Middle of the *Humerus*, *Tibia*, and *Perone*, by the Power of the Muscles only, or such Power of them as sufficiently shew'd they have as great a Share in several Fractures ascribed to outward Causes, as they are frequently known to have in the Fracture of the *Tendo Achillis*. However, as the Fractures occasion'd by the Action of the Muscles are cured by the same Remedies as are effectually applied to the Cure of others from outward Causes, they deserve no further Notice. I shall give some Instances of such in which the Bones themselves are morbid.

Upon the 15th Day of *July* 1738. I was sent for to a middle-aged Gentlewoman in my Neighbourhood, of a seemingly strong Constitution, rather fat than lean, who had broke her Right Arm in the Middle, while with her two Hands she was endeavouring to bring together the Ends of a Piece of Tape. The Standers-by heard the Bone snap, and were

were the more astonished at the Cause, as the Patient was sitting distant from any thing that could strike her Arm, or contribute to the Breaking of it. The Grating of the Ends of the Bone against each other, when the Arm was moved, the Loss of its Action, and all the common Attendants of a Fracture, were sufficient convincing Proofs of it. It was reduced with Ease, and dressed as usual; but, I own, I doubted of the Cure, till Mr. *Shipton*, who was consulted thereon, assured me, that he was Witness to several Fractures of this kind, where the Patient had done well. We agreed, that the Bone must have been distempered, and likely with a Cariosity; it appearing otherwise hardly credible, that the Bone could have been broke from so slight a Cause as this.

I have known two Patients die after a Fracture of the *Os Femoris* from the like; the first snapping in the middle as he was getting into Bed, in whom all the Centre of the Bones was carious; and the latter getting out of it, wherein only a small Part was so, and above two Thirds of the Circumference in every respect appear'd sound.

For two Years last past my Patient, had been attended by Mr. *Shipton*, on account of several scorbutic Complaints, which I thought proceeded from some Venereal Taint; but the Patient vowed, and hath all along assured us, she never had known Man.

Two Years before this Accident happened, I had attended this Patient jointly with the late Mr. *Fiquel*, on account of a *Spina ventosa* in the Centre of the *Os Bregmatis* on the right Side; which being laid bare, the *Caries* was found to have pene-

trated the two Tables thereof, and all that time she had a gummatous Swelling upon the upper Head of the *Radius* on the right Arm, checking the Motion of this Bone in Pronation and Supination, both which she then mentioned to have appeared, upon the Retreat or Disappearance of broad yellow Spots (not scarfy, nor in any wise resembling those in the *Lues venerea*) her whole Body had been bespecked with for some Years last past; she having all her Life been addicted to the Scurvy, and never been in the Way of getting a bad Distemper; neither were the Pains she complained of so vexatious to her during the Night as the Day.

Upon Mr. *Fiquel's* Death, for two Years last past, she had, by Mr. *Shipton's* Advice, taken the most powerful Antiscorbutics; notwithstanding which, the forementioned Humour on the upper Head of the *Radius* was much increased, and a new one of the same kind, for some Months last past, had appear'd on the Head of the Shoulder-Bone, and another on the Middle of the Bone where the Fracture now was. She likewise reported, that she hardly was cured of the *Caries* I had attended her for on the *Os Bregmatis*, but another Tumour had appeared on the Skirts of the *Os Frontis*; which breaking of itself, the Bone there had been found carious, whence she had had a Running ever since; and that of late had appeared two puffy Swellings over the left Brow, of the like Nature with those that had broke before, and therefore fear'd the Bone there would also prove bad; and that there was also a hard Swelling, attended with Pain, springing up upon the Head of the *Radius* on the left Arm.

As all the Remedies hitherto used had proved ineffectual, Mr. *Shipton* and I concluded, that a Mercurial Salivation was the most likely to conquer a Humour daily laying hold of new Bones; but we thought it improper to put the Patient into it, whilst we stood in need of a *Callus* for the Knitting of the broken Bone, The Cure of it has been completed in about six Weeks, and the Patient has ever since had the full Use of it.

At this time we had an oedematous *Phlegmon* over the left Brow, with a Fluid undulating there in two Places; and, by way of Addition to the former Complaints, a Node also was springing up with Pain on the Head of the *Radius* upon the left Arm, attended with an oedematous Swelling. In this Condition she was put into a Salivation by Uñction.

This Salivation, copious as it was, was kept up a whole Month: In the first Fortnight of it, the Matter, gathered in the oedematous *Phlegmons* over the Brow, intirely disappeared, and then the *Oedema* and Tumour in the Bone upon the Fore-Arms where the Nodes were; and these being nearly dissolved in the last Fortnight, we were in Hopes the Cure had been brought about without opening the Tumours over the Brow, where Matter had been felt, and the Bone was thought carious: But our Joy was not lasting, many of the Complaints re-appearing in a short time after, notwithstanding we took the utmost Care to confirm the Cure by Salivation, by a subsequent Course of Antiscorbutics, the Decoction of the Woods, &c. For as, upon the Retreat or Disappearance of the yellow Spots in the Skin, first-mentioned, the Dislemper in the Skull had begun,

and the Nutriment of the Bones in general had thereupon been vitiated, as appeared by the gummatous Tumours, and *Nodus's* on the Bones, the *Spina ventosa* in the Skull, and the Extravasations of Matter, denoted by the œdematous *Phlegmons* upon the Brow and *Radius*; so it appeared no less evident, that the Retreat of these, and the Reflux of the Matter into the Blood, had contaminated it again, being the forementioned Symptoms, and worst, re-appeared within less than three Months after: They had disappeared very fast, as the Vessels were emptied during the Salivation, but they re-appear'd again faster upon the Filling.

During the Salivation, or soon after, the Cariofities in the Skull, that had been laid bare, having been exfoliated, were cured; the *Nodus's* upon the Shoulder-Bone, and *Radius* on the Arm that had been broke, had vanished away; and, what was thought no less remarkable, the Matter extravasated upon the right Side of the *Os Frontis* over the Brow, where the œdematous *Phlegmon* had appeared and disappeared at times, were intirely dispersed; and these, saving the Tumour on the *Radius* of the left Arm, never appeared again.

The Renewal of the Distemper shew'd itself upon the Bones that had been laid bare, by a new Cariofity spreading very fast in the Neighbourhood. At this time an inflammatory *Oedema*, that had appeared upon the *Os Frontis* over the right Brow (very distant from that Collection that had been made on the left before the Salivation, which had been dispersed, and now continued well) suppurating, was opened, and the Matter found to spring from the
frontal

frontal *Sinus's* on this Side thro' the Bone that was carious. And as the Tumour that had lately appeared upon the *Radius* of the left Arm, and flood out the Salivation, was now increasing with Pain, with an oedematous *Phlegmon* upon the Arm where the Tumour was; it was agreed to put her forthwith into a second Salivation no less copious than the first, but of a longer Continuance; to denude, first, all the frontal and parietal Bones on the right Side, that were carious, but to leave untouched those on the left Side; which, before the first Salivation, had been suspected of being a *Spina ventosa*, but now appeared sound.

The Patient enter'd into this Salivation about the latter End of *May* 1739, after she had been duly prepared to it. This was continued ten Weeks, because of the frequent Interruption we had met with by the Returns of a *Diarrhœa*, as oft as she was anointed, and the profuse Sweats during the *Dog-Days*. In this, as in the former Salivation, all the Accidents gave Way during the Course. The Progress of the Cariosity in the Bones of the Skull was stopped, and the Exfoliation being made, the Wound was cured afterwards. She was put into a Milk Course, and sent into the Country for the Recovery of her Flesh; and after that into a subsequent one of a Decoction of the Woods, &c. but the Advantage she had reaped by these did not continue long.

In the Autumn, she was frequently traversed by irregular Shiverings and Rigors, upon the Re-appearance of the phlegmonous *Oedema* about the *Node* still subsisting on the left Arm; which now again grew larger

larger with Pain in it, still increasing in proportion; as the Vessels emptied in the Salivation were replenished. The oedematous Swelling about the Tumour was also more phlegmonous, and Matter was forming there on the Bone, which, it was apprehended, was a *Spina ventosa*: I therefore insisted upon the Laying of that open: But, whilst she was preparing for her Removal to Town, this Tumour intirely disappeared upon the Appearance of a *Diarrhœa*, that sunk her too fast to admit either of her Removal, or any Operation. This followed her to her Death. During the two last Days of her Life, she was in a constant *Delirium*, and universal Convulsions.

The Patient dying thus in the Country the 25th of *September* last, I was not acquainted with these latter Accidents till after her Burial; and therefore missed the Opportunity of dissecting the Body, which perhaps might have given some additional Light to this Case: But, from so much as has been reported of it, I think it may be inferred,

i. That if a scorbutic Humour occasioned the Complaints in this Case, as there is Reason to believe it did, from the Veracity and Character of the Patient; and those Symptoms being wanted that usually appear in the *Lues Venerea*; yet it must be confessed the Affection in the Bones yielding to a Mercurial Salivation, as these did, and the Return of Complaints upon the Reflux of the Matter extravasated, contaminating and tainting the Blood with a new Ferment, or the same as before the Flux, may ground a strong Suspicion, that those in our Patient proceeded from some Venereal Taint.

2. That a Siccity or Brittleness in the Bones, inclining them to a Fracture, may happen, independent from a *Caries* in the Bone: For, had the *Os Humeri* of our Patient been carious, at the time the Fracture happened from so slight a Cause as attended the Endeavour of bringing and joining together the opposite Ends of a String held in the two Hands, it was morally impossible the complete Cure of such a Fracture could be brought about within six Weeks. Whence it appears, that the occasional Siccity of a Bone may be no Bar to the Flowing of that Quantity of Sap or callous Matter necessary to operate the Cure after a Fracture.

3. That the Muscles must have a great Share, as well in the Fracture of all Bones, as in the Dislocations of them; as in this Case the *Os Humeri* was broke by the Power and Action of the Muscles only; and therefore that the keeping of them quiet, by placing the broken Limb in the most natural and easy Posture, must be highly instrumental to the Cure.

4. That the Absortion, or Reflux into the Blood of a purulent *Sanies*, inflating the Part where it is lodged under the Appearance of an oedematous Phlegmon, or phlegmonous *Oedema*, is as dangerous as the Absorption by the Blood-vessels of a purulent Matter extravasated. So that the Reflux in either Case will again taint the Blood with the Corruption that had critically been flung out of the Course of the Circulation; and that, when this happens, there will be Cause to fear a Renewal of the Complaints, and, possibly, worse Symptoms; as has happened in the present Case.

3. That yet there appears a wide Difference betwixt a *Phlegmon* œdematous and an œdematous *Phlegmon*; inasmuch as, in the first, the *Oedema* goes off as the *Phlegmon* comes to a Crisis; whereas, in the latter, that seldom comes to a Crisis, but changes, and appears and disappears continually. When the Matter of a *Phlegmon* prevails, the Tumour may be resolved, without any Inconveniency to the Patient, if it is not critically determined upon the Crisis of a Fever; and when it is, the Depuration being complete, he fares the better for it: Whereas, in the œdematous *Phlegmon*, wherein the *Serum* prevails, the Tumour beginning with an *Oedema*, neither the Tumour, nor the Inflammation of it, are permanent; but appearing and disappearing, as the sanious Matter is thrown out of the Blood, or refluxes back into it. The sanious Matter of it is apt to contaminate the whole Mass, as oft as the Tumour disappears; and therefore it is evident, that, in this latter Case, that is in the *Oedema* phlegmonous, we are to give a Vent to the Matter lodged in the Parts as soon as may be, and even before the Matter is concocted, or fully collected, as in critical Abscesses; viz. so soon as some irregular Shiverings, and such other Symptoms, have denoted the Extravasation of the Matter in the *Membrana cellularis*, wherein the Humour first makes its Appearance. For that when in an *Oedema* phlegmonous, or œdematous *Phlegmon*, the Matter changes, at times, with more, and at others with less Inflammation; the Tumour increasing and diminishing alternately, as the Matter becomes more or less sanious and purulent, and refluxes into the Blood at times: We may then fear some Lodgement of it will be made upon
some

some of the *Viscera*, if that again is not critically thrown out: And if it remains in the Mass, that, in the Course of the Circulation, it will occasion such Symptoms as here have happened to our Patient, during the Course of the Distemper, even to her last, as oft as that has happened; and that in so plain a manner, as to make it evident, that the Renewal of the Symptoms was consequential upon the Reflux of a purulent *Sanies* back again into the Blood from the Part wherein it had been lodged: And thus it appeared, that as this Reflux of Matter was principally made during the Salivation, when the Vessels emptied could best attract it; so it was expedient the Discharge of it had been made before that was enter'd upon.

5. The Return of the Complaints from this Cause was very obvious at last, but not so at first: And should not this make us tender and circumspect, when we pass a Judgment upon the Conduct of others? That the Matter should so shift its Lodgments after every Salivation, is no less remarkable than that it should rather fix on new Parts, than those which had been affected before; and that the Cause of it, virulent as it was from the first, and attacking the Juices flowing in the Bones in so particular a Manner, should have been no Check to the complete Cure of the fore-mentioned Fracture in the usual Time. It were to be wish'd Men of Experience were more ready in shewing the Errors, Failings, and Slips in their Practice than their Successes: That might be of great Service to the Public, and those Warnings prove of singular Benefit to those who have not had the like Opportunities, it being more eligible to be

informed by the Failings and Misfortunes of other than one's own.

XVI. *An Account of an extraordinary Case of the Bones of a Fœtus coming away by the Anus; communicated by John Still Winthrop, Esq;.*

Read March 14.
1744-5.

THERE are several Instances of the Bones of *Fœtus's*, which have died in their Mothers Bellies, making their Way out by preternatural Manners; some by the Navel, some by the Groin, and some by the *Anus*. Of this last sort I am now going to give another Instance, which happened in *New London* in *New England*, in the Year 1737. A Negro Wench was thought to have conceived with Child; and about three Months after, she had some Appearances of a Miscarriage, but no *Fœtus* was observed to come away. This therefore made the good Women now alter their Opinion; thinking that she was not with Child, but only had not been regular from having taken Cold: Whereupon Remedies, proper in such a Case, were given her; but she found no Relief from exceeding great Pains she complained of in the Bottom of her Belly, and in the Small of her Back, more particularly when she went to Stool: Her Flesh wasting extremely, a skilful Woman was sent for, who found Milk in her Breasts, and other certain Tokens of her being with Child. She continued wasting in a miserable Condition, growing less in her Belly, and her Breasts falling,

falling, and was at last given over: But at length, at the End of about eight Months, she brought away much Blood by Stool, on which her Pain in those Parts abated; and then she voided with her Stools these Bones with Flesh and rotten Skin about them. After this she soon grew well, and recovered entirely. All the Parts of the *Fœtus* were found in her Stools, except the Head; which is supposed to have come away by the *Vagina*, when she had the Symptoms of Miscarrying above mentioned; for it was now recollected, that she then said, Something came away with her Water as big as a large Nut, but it was not then attended to. As the Case was very remarkable, so I hope it will prove acceptable; tho' not drawn up with that Accuracy with which a Physician might have done it.

XVII. *A Letter from Charles Jernegan, M. D. Lic. Coll. Med. Londin. to Cromwell Mortimer, M. D. Secr. R. S. concerning an extraordinary Cystis in the Liver, full of Water.*

S I R,

Read March 14. 1744-5. I HOPE you will excuse the Liberty

I take in communicating to you an uncommon Case or Disorder of the Liver, lately observed at the opening the Body of Mrs. A. B. deceased,

aged near Forty, whom I had attended some Weeks before. The Complaint was a constant acute Pain on the Region of the Liver, with a Swelling, or more than ordinary Fullness on that Side; by pressing of which was perceived a Fluctuation of some Fluid lying deeper than just under the first Teguments. This was confirmed by Mr. *Sherwood*, the Surgeon who assisted and examined the same. The Body was opened by his Son Mr. *Sherwood* junior, when the Liver was found of a prodigious Size (there was a small Adhesion to the *Peritonæum* without Inflammation): It spread over the Stomach quite to the Spleen on the left Side, and contracted much the Cavity of the *Thorax*, by pressing and thrusting up the *Diaphragma*. On opening the great Lobe of the Liver, there issued out above four Quarts of a limpid Water, from a Cavity formed by the proper containing Coat of the Liver; tho' the Water itself had been contained in a single conglobated Gland, and there formed a *Cystis*, which had burst, and was found loose at the Bottom of this large Cavity. This Skin or *Cystis* was not so thin but still capable of further Expansion.

The Liver still did its Function of separating the Gall: The Gall-bladder and its Ducts were in a good State: The *Lobulus Spigelii* was much enlarg'd, and crumbled easily like a Mass of congealed Blood.

The Patient had no particular Thirst; nor was there any Alteration in the Urine, as to Quantity more or less. But she had this Symptom, common in the *Hydrops Pectoris*, of not bearing any other Posture

Posture but that of leaning forwards on her Breast. I
am, with all Respect,

S I R,

March 10. 1744.

Your most obedient

Humble Servant,

Ch. Jernegan.

The left Kidney, being longer than usual, was
examined and found to have two Ureters; and
each had its separate *Pelvis*.

XVIII. Regiæ Societati *Anglicanæ* Scientiarum
quædam Electricitatis recens observata ex-
hibet *Jo. Henricus Winkler*, Gr. & Lat.
Literarum Prof. Publ. Ordin. & Academiæ
Lipsiensis h. t. Rector.

I.

Tritu excitatæ Electricitatis Genera.

Presented March 21. 1744-5. § 1. **V**ITREI cavi que globi, &
vitrea vasa, quæ facta
rotatione applicataque iis manu teruntur, in sibi vici-
nis metallis atque hominibus cum electricitatem ex-
citant, ut scintillæ electricæ, quæ accedente corpore
electricitatis vacuo eliciuntur, fluminis instar continu-
atæ prorumpant.

§ 2. Si vero tubi & vasa vitrea ita teruntur, ut itum reditumque subeant, excitata inde in metallis & hominibus electricitate, oriundæ scintillæ per intervalla profluunt.

§ 3. Ad tubos eommode terendos machinam parari curavi, quam TAB. III. Fig. 2. repræsentat.

Tabulæ *a b c d* quatuor columnæ inferuntur. Mediarum *e* & *f* capitibus *g* & *h* ope cochlearum afferculi affiguntur, quorum pars media ita cavata est, ut convexitati tubi vitrei congruat. Cum his afferculis alii ejusdem generis afferculi pariter cavati vi cochlearum junguntur. Columnam ejusmodi cum impositis junctisque afferculis Fig. 3 exhibet, ubi *i k* afferculum inferiorem, & *l m* afferculum superiorem, & *n o* cochleas firmantes ostendit. Superioris & inferioris afferculi cavaturæ, corio cervino subjectisque erinibus obductæ & vestitæ ita congruunt, ut tubum vitreum, qui hinc inde trahi potest, arcte complectantur. Tubi vitrei extremitates *q q* capsulis ex aurichalco paratis induuntur, firmanturque maltha factitia.

Capsulis annuli annexi sunt, quibus illigantur funes cannabini, quorum alter *q r* per foramen columnæ *t u*, alterque *q s* super trochleam *x* columnæ *y z* affixam protenditur. His ita constitutis, tubus vitreus, quando a duobus hominibus ultro citroque trahitur, excitatam in se tritu electricitatem cum tubo ex lamina ferrea confecto *a b* & in rotibus sericis collocato large communicat. Tubi ferrei extremitati *a* fila argentea alligantur, quæ tubum vitreum inter duas columnas *e g* & *f h* attingunt.

§ 4. Quamvis vero scintillæ globo vitreo rotato excitatæ in metallorum superficiebus continuo fluant, eæ tamen, quæ a vasis vitreis itum reditumque inter

terendum subeuntibus proficiuntur vehementius pungunt, si vasa eandem, quam globi magnitudinem habent, paremque materiæ vitreæ bonitatem.

§ 5. Porro scintillæ electricæ, quæ tractis tritisque tubis vitreis in metallorum superficiebus suscitantur, pungendi virtute superant scintillas excitatas vasīs vitreis, quæ more tornatorum teruntur.

§ 6. Globi vitrei manu applicata inter rotandum triti plus electricitatis exhibent, quam adhibito pulverinari corio vestito.

§ 7. In experimentis, quæ aut globo rotato, aut tubo tracto instituuntur, tribus hominibus opus est. Adhibita vero machina tornatoria, sufficit unus.

II.

Electricitatis agenda Ratio.

§ 8. **U**NO vel vase, vel globo, vel tubo vitreo excitata electricitas, mihi simplex vocatur. Fit duplex, duobus vel vasis, vel globis, vel tubis tritis; triplex tribus; quadruplex quatuor, & ita porro.

§ 9. Quam excitavi electricitatem tritu duorum globorum vitreorum, quorum diametere est pes dimidius *Parisinus*, tanta fuit in aqua, in nive, in glacie, ut prorumpentes ex his corporibus scintillæ electricæ spiritum vini purum calefactumque inflammarent.

In aqua experimentum dupliciter capitur. Vel enim spongiæ aqua impletæ, atque ex lamina ferrea in speciem tubi conformata & electricitatem nanciscente pendentis, spiritus in parvo cochleari adhibetur; vel digitus spiritu vini calefacto madidus super aquam in vase stanneo extenditur, certo tamen inter aquæ superficiem

superficiem & digitum interjecto intervallo. Vasi, serico reti imposito, adjungitur filum ferreum, quod ad globum aut tubum aut vas vitreum in machina electricitatis perringit. Nix & glacies itidem in vase stanneo reti serico imponuntur.

§ 10. Ut electricitas adhuc major existat, duæ machinæ ita collocantur, ut quælibet duos habeat globos, qui electricitatem cum uno eodemque tubo ferreo communicant. Quod quomodo efficiatur, TAB. III. Fig. 1. significat. Cuivis machinæ apponitur rete sericum *ab*, quocum tubus ferreus *cd* conjunctus est, qui prope utramque machinam duo brachia ferrea *bc* & *ef* atque *bd* & *gh* porrigit, quibus annexa sunt fila argentea, quæ globos in locis *iklm* attingunt.

Si loca globorum vasa vitrea occupant, quæ applicatis pulvinaribus teruntur more tornatorum agitata: brachiis ferreis nulla adjungo fila argentea, quæ vasa attingant. His enim ita adjunctis electricitatem minorem existere deprehendi.

III.

Electricitas, quando ex Tubo ferreo in Corpus, ex quo primitus orta est, redit, imminuitur.

§ 11. **E**Xperimentum sequenti ratione instituitur. Machina cum vase vitreo, & homo, qui more tornatorum calcando yas vitreum agit, insistant retibus sericis adeo amplis, ut & machinæ & hominis calcantis pedes a lateribus ligneis, quibus retia alligata sunt, longissime distent.

§ 12. Quando vas vitreum agitaturn pulvinari atteritur, non solum tubus ferreus in reti positus vasi- que proximus, sed homo etiam & machina electricitatem certam produnt, qua in orbe vitreo, quem alius homo, in reti serico non insistent, manu tenet, sub- jecta corpuscula leviora varie commoventur.

§ 13. Idem fit, si loco vasis globus adhibetur; & homo, qui ipsi manum inter rotandum applicat, uno pede machinæ, alteroque reti serico insistit.

§ 14. Si vero, his omnibus ita constitutis, tubo ferreo *ab* (TAB. III. Fig. 10.) in reti serico prope vas vel glo- bum vitreum collocato alius *cd* adjungitur ita exten- sus, ut machinam v. c. in loco *e* attingat; scintillæ, quæ ante excitari poterant, desinunt, ipsaque vis at- trahendi quam maxime imminuitur.

IV.

Electricitas in vacuo.

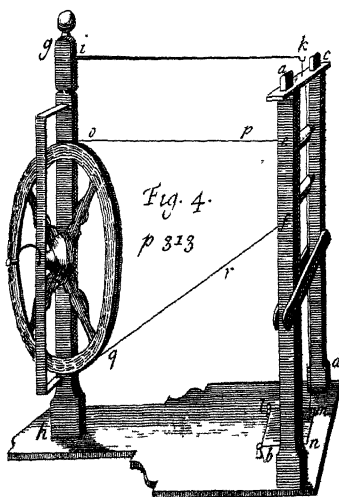
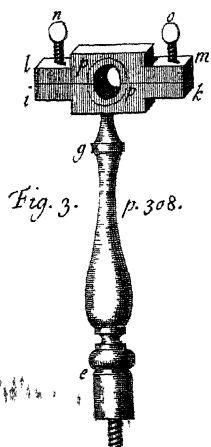
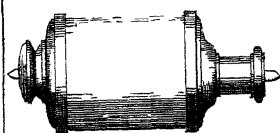
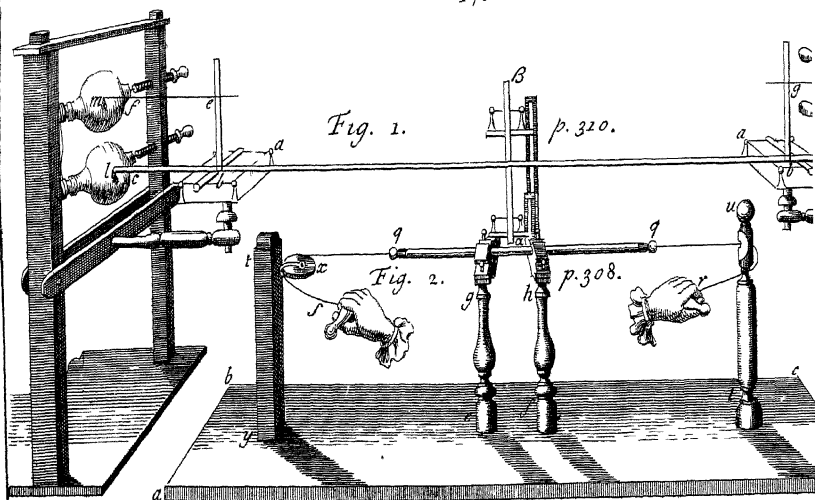
§ 15. **M**Achina, cujus ope electricitas in spatio vacuo commode excitari, & per cam- panam vitream in aërem propagari, & cum corpori- bus omnis generis communicari potest, in TAB. III. Fig. 6. exhibetur.

Constat vase vitreo *abcd* (Fig. 8.), cujus basibus *ac* & *bd* malitia faditæ junctæ sunt laminæ ex auri- chalco confectæ, quarum uni *ac* annexum est brachium ligneum *ef*. In hoc brachio igneo alteraque lamina *bd* cavaturæ sunt conoides, quibus axiculi indi possunt, qui cochleæ formam habentes infixi sunt lateribus, sustentaculi metallici *ghiklm*, quod cochlea mare *mn* instructum cochleæ scemellæ in orbe antliæ

pneumaticæ inferi potest. Cochlea mas transit per foramen laminæ curvatæ elasticæ, quæ in *Fig. 9.* describitur. Sustentaculi pedi (*Fig. 7.*) *lm* vi cochleæ affixa est lamina *no*, cujus pars superior *pq* corio cervino subjectisque pilis obducta vasi vitreo adjacet.

Quæ superimponitur, campanæ *abcd* (*Fig. 6.*) infixus est cylindrus metallicus perforatus *g*, per cujus foramen chorda ex intestinis animalium contorta trajicitur. Hæc chorda intra campanam circum brachium ligneum *ef* (*Fig. 6.*) circumligatur; & fibula, quam annexam habet, laminæ curvatæ elasticæ in fine perforatæ *rst* innectitur. Extra campanam chorda per vesicam suillam utrinque apertam transmittitur. Altera pars vesicæ circa tubulum metallicum *g*, in quo extrinsecus sulculi circumducti sunt, circumligatur, filoque cannabino adstringitur; altera vero *u* inter duos nodos in chorda nexos arte colligatur. Vesica madefactione ita paratur, ut, postquam intrinsecus linteò detergita fuit, distrahi contrahique se facile patiatur. Extra vesicam certa pars chordæ *ux* eminet, qua arrepta & tracta vas vitreum sub campana agitari terique potest.

§ 16. In vasculo quadrato ex lamina ferrea confecto *αβγδ* (*Fig. 6, 7, 8.*), quod vel in reti serico supra vas vitreum cavumque *abcd* (*Fig. 8.*) extenso, vel in resina colophonia, vel lacca signatoria positum est, ferreumque stilum *γε* versus pulvinar protensum sibi annexum habet, ténues auri particulæ collocantur. Mobili cylindro metallico, *ζη*, qui per medium campanæ collum protrudi potest, transverse annexum est filum ferreum *ηθ*, duas aut tres lineas distans a particulis auri. Hæ versus illud affiliunt, simulac



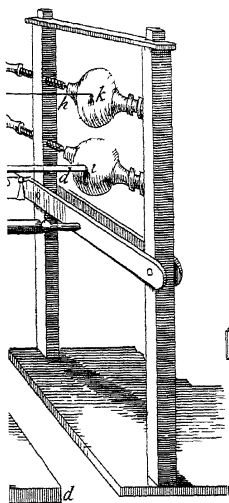


Fig. 7. p. 312

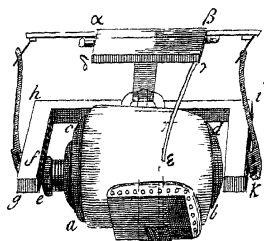
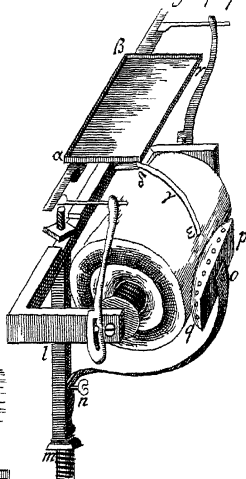


Fig. 8. p. 312.

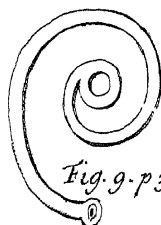
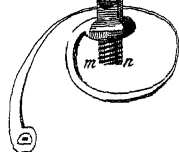


Fig. 9. p. 312.

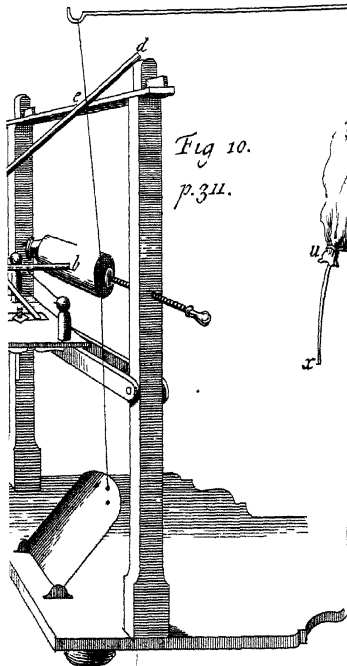


Fig. 10.
p. 312.

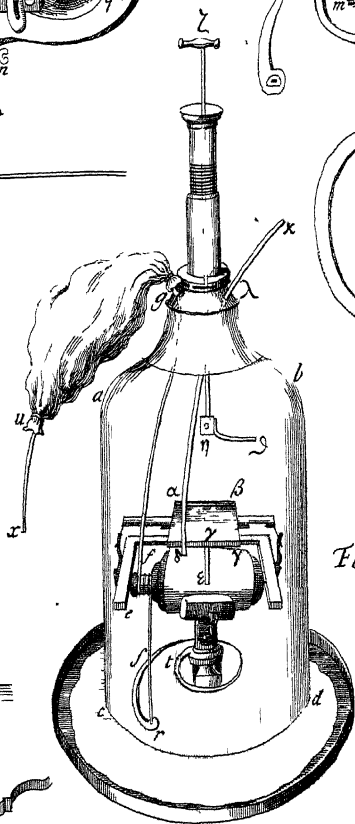


Fig. 6. p. 312.

simulac vas vitreum, aëre ex campana educto, agitur & pulvinari atteritur.

§ 17. In altero campanæ latere perforato λ tubulus vitreus infixus est, per quem filum ferreum $\alpha \lambda \delta$ ad medium vas vitreum pertingit, parvo admodum inter vas & filum interjecto intervallo. Tubulus pariter ac filum liquefacta lacca signatoria ita firmanantur, ut nullus aer penetrare valeat. Qui ut omnino arceatur, cylindrus mobilis $\zeta \eta$, ubi collum campanæ attingit, sebo circumfunditur. Tracta chorda $\alpha \eta g$, filum ex agitato tritoque vase electricitatem non solum concipit, sed etiam per tubulum vitreum liquatione obturatum propagat, & cum corporibus in serico positis, quæ forinsecus filum ferreum in loco α attingunt, ita communicat, ut metalla in tenebris scintillas electricas emittant, appropinquantibus corporibus electricitate vacuis.

§ 18. Ita etiam electricitas forinsecus excitata cum filo illo ferreo communicatur, & per tubulum obturatum pervadit, & in fine fili intra campanam lucem in tenebris effundit, ac tenues auri particulas in vasculo ferreo collocatas concitat.

V.

Ufus Machinæ TAB. III. Fig. 4. descriptæ.

§ 19. **I**Nter duas columnas anticas ab & cd vasa vitrea vel globi vitrei e & f suspenduntur, & columnæ tertiæ posticæ foramini superno lamina elastica ik inditur, & lateri rota adjungitur. Laminæ elasticæ in k annexa chorda ex intestinis animalium confecta circum longiora vasorum brachia circumligatur, & asserculo mobili $blmn$ annectitur. Ita vasa vitrea more tornatorum agitari possunt.

§ 20. Si autem vas vel globus rotari debeat, funis *opqr* circum rotam & sulcos ligneos vasorum aut globorum circumducitur, qui ope cochleæ in parte machinæ postica applicatæ & tendi potest & remitti.

§ 21. Columnas anticas coercent & firmant duo juga, ex quibus duo cylindri perforati eminent, quorum postico parva columella infigitur, in qua duo rursus cylindruli corio cervino cum subjectis pilis obducti cochleis firmantur; antico autem instrumentum, in quo fila serica extensa sunt, quibus tubus ferreus cum duobus brachiis alligatur. Hunc tubum homines apprehendunt, qui retibus sericis insistentes electricitatem nancisci cupiunt. Tubo illi si inditur ensis, cujus capulum ex filo serico pendet; ex ejus umbone, excitata electricitate, scintillæ electricæ profluant, spiritum vini in parvo cochleari inflammantes Sic stella*, quam electricam (*Fig. 11.*) voco in reti serico majori ponitur, & ope fili ferrei cum tubo vitreo brachiato, annexo reti minori prope vasa vel globos, conjungitur. Simulac vasa vitrea, vel adjunctis pulvinaribus, more tornatorum agitantur, vel adhibitis manibus, rotantur; stellæ radii in tenebris lineas lucentes emittunt, & facta stellæ conversione, circulum lucentem describunt.

§ 22. Quando vasa rotantur, brachiis tubi ferrei adjunguntur argentea fila, quæ vasa attingunt. Ita flumen electricitatis continuum elicitur. Contra autem minuitur electricitas, si vasorum extremitates, facta ad modum tornatorum agitatione, adjuncta habent fila argentea, quæ vasa attingunt. Pari modo, si
vasis

* *Vide Asta Germanic. or The Literary Memoirs of Germany. Vol. II. p. 123.*

vasis rotatis pulvinaria loco manuum applicantur,
electricitas decrefcit.

XIX. *A Description of a Machine to blow
Fire by the Fall of Water ; by James Stir-
ling, F. R. S.*

See TAB. I. Fig. 6.

Read March 21.
1744-5.

ABCD is a Pit dug in the Ground, whose Surface is higher at *D* than on the other Side at *A*. The Bottom *BC* is strongly ramm'd with Clay, upon which are laid thin fawen Deals.

In this Pit is fixed a Tub *GHI* without a Bottom, having a Hole *I* at the lower Part of the Side, and all round the Tub is ramm'd with Clay, except at the Hole *I*.

In the middle of the upper End of the Tub is fixed a Pipe *PQRS*; at the higher End of which are four Holes pointing downwards, whereof two are represented by *S* and *R*.

SRTU is a Funnel fixed on the Top of the Pipe, with a Throat *XZ* narrower than the Bore of the Pipe. In the upper End of the Tub towards one Side is fixed a crooked Pipe at *LM*, tapering to the End at *N*. It is made of Wood so far as *O*, but from *O* to *N* of Iron, the Fire being supposed at *N*. *EF* is the Surface of a plain Stone, raised up in the middle of the Tub, directly under the Pipe *PQRS*.

The

The running Water, being let in at the Top of the Funnel, falls thro' the Pipe upon *EF* the Stone in the Tub; it runs out at the Hole *I*, but cannot get off till it rises as high as *A*.

This raises it in the Tub almost up to the Surface of the Stone, and it must not rise higher.

So much Water must run in at the Top of the Funnel, as will keep it always full, or nearly so.

This Height of Water squeezes it into the Pipe with a great Velocity; but, since it passes thro' the Throat of the Funnel, which is of a smaller Bore than the Pipe, Room is left all round the Vein of Water for the Air to enter at the Air-Holes.

It no sooner enters but it mixes with the Water, on the Account of the Rapidity of the Motion; and both together make a white Froth, and intirely fill the Bore of the Pipe. When this Froth falls on the Stone in the Tub, it is dashed into small Particles, which disengages the Air from the Water. The Air cannot get out at *PQ*, the End of the Pipe, because it is fill'd with the Froth, which falls with a great Force; neither can it get out at the Hole *I*, because the Surface of the Water is kept so high above it; and for that Reason it rushes out at *N*; and if the Hole *N* be stopped, the Air will soon force all the Water in the Tub out at *I*, and then follow it.

The most convenient Way of regulating the Blast, is to bore a small Hole in the Blast-Pipe; and, by the Help of a Pin in it, to let out what Air there may be more than is wanted.

The Dimensions of such an Engine sufficiently big to smelt harder Ore than any in Lead-Hills, are set down at the Bottom.

Feet.

	Feet.
Height of the Funnel - - - - -	5
Length of the Pipe - - - - -	14, 15, or 16
Height of the Tub - - - - -	6
Diameter of the Tub - - - - -	$5\frac{1}{2}$
Height of the Stone in the Tub -	2

	Inches.
Diameter of the Throat of the Funnel - -	$3\frac{1}{2}$
Diameter of the Bore of the Pipe - - -	$5\frac{1}{4}$
Diameter of the Blast Hole at <i>N</i> - - -	$1\frac{3}{4}$
Hole at <i>I</i> about 5 Inches square,	
Diameter of the Air-Holes - - - - -	$1\frac{1}{2}$

This Engine is likewise of admirable Use to convey fresh Air into the Works; which saves the double Drifts and Shafts, and cutting Communications between them.

A small one will do very well for a Blacksmith.

XX. *A Letter from Dr. John Lining, at Charles-Town in South Carolina, to James Jurin, M. D. Coll. Med. & Reg. Soc. Sod. serving to accompany some Additions to his Statical Experiments printed in N^o. 470. of these Transactions.*

South Carolina, Charles-Town, Jan. 29. 1743.

S I R,

Read March 21.
1744-5.

AS you did me the Honour to communicate to the *Royal Society* the Tables which I sent you some time ago, and since some Excerptions from them have been published in the *Transactions* of that illustrious *Society*, N^o 470. I have taken the Liberty to send you the inclosed Papers; which, being more complete, I hope will be acceptable.

The first second and third Tables* are the same with what I sent you at first, [Printed *ibid.* TAB. I. p. 502. & TAB. IV. p. 506, 507.]: And in the third I have supposed the *Ingesta* each Month to be 3000 Ounces, to coincide with two Columns in my *Sanctorian* Tables, where I have daily supposed the *Ingesta* to be 100 Ounces. [This answers to TAB. V. *ibid.* p. 505. but I shall here give it anew, p. 321. *infra*] The mean Quantities contained in the fifth Table, [p. 323. *infra*] were obtained by a tedious operose Calculation; namely, by calculating each Day's mean diurnal and nocturnal Urine and Perspiration of one Hour in every

* Therefore omitted here.

every Month; and by dividing the Sum of each Month's mean diurnal Perspiration, &c. by the Number of Days in each Month. The mean diurnal and nocturnal Heat by *Fahrenheit's* Thermometer were taken by the same Method.

The Deductions from the Table, p. 323. *infra*, I have digested all that I could of them tabularly, that they might be as clear and short as possible, and appear to me to point out the physical Principles, from whence we may account for the Production of these epidemic Diseases of the different Seasons, which are not infectious. For are not these the Effects of different Constitutions of the Air on human Bodies? And are not the Increments and Decrements of the sensible and insensible Excretions, Regard at the same time being had to the Quantity and Quality of the *Ingesta*, and to the Exercise, &c. the only *Index* of the Changes produced in the human Constitution, by the Vicissitudes of the Weather?

That indeed was the only View I had in going thro' these troublesome Experiments with so great Assiduity for one whole Year. The Design was benevolent, and I am afraid far superior to my Capacity, especially as I am situated in a Place where I can have no immediate Assistance.

That I may be furnished with as many *Data* as possible, I propose to take the specific Gravity of the *Cruor*, of the *Serum* and *Crassamentum* of the Blood, in different Diseases, and in their several *Stadia*, by a very nice hydrostatic Balance, made by Mr. *Jackson*. But this indeed is attended with greater Difficulties than I was at first apprised of; for the Experiment requires a greater Quantity of Blood

S f

than

than can at all times be safely taken away; and Rain-Water, with which the specific Gravity of the Blood is compared, I have found, by repeated Experiments, to lose about $\frac{3}{112}$ Parts of a Grain for each Degree of Heat by *Fahrenheit's* Thermometer; and Oil of Turpentine, in which the *Crassamentum* is weigh'd, loses much more of its specific Gravity.

If I can possibly obtain the same kind of Instruments which Dr. *Langrish* used, I would likewise take the Cohesion of the Blood, and analyse it and the Urine in different Diseases, &c. but the Difficulty of getting exact Instruments is very great.

That you may enjoy long Life, and perfect Health, to improve that Science in which you are so worthily exercised, is the sincere Wish of,

S I R,

Your most obliged,

and most humble Servant,

John Lining.

AS the Quantity of *Ingesta* varied considerably in almost every Month, the Increase and Decrease of the several *Excreta*, as they were influenced by that Constitution of the Air, which is exhibited in TAB. IV. p. 506, 507. *Transf.* N°. 470. cannot plainly appear. I have therefore calculated the following Table from that, supposing the mean *Ingesta* of each Month to be 3000 Ounces, which is equal to 100 Ounces each Day; and that the several *Excreta* were evacuated in the same

and

Ratio

Ratio from these 3000 Ounces, as from the real Quantity of each Month's *Ingesta*: Whence the Increment or Decrement of the sensible and insensible Evacuations, in each Month, as they were influenced by the Weather, will be more conspicuous.

	Urine	Persp.	Stools	Urine		Perspirat.		Stools	
				Increased.	Diminished.	Increased.	Diminished.	Increased.	Diminished.
March	1799	1106	91	—	—	—	—	—	—
April	1618	1323	91	—	181	217	—	—	—
May	1731	1492	87	—	187	169	—	—	4
June	1254	1706	91	—	177	214	—	4	—
July	977	1941	88	—	277	235	—	—	3
Aug.	1274	1628	98	297	—	—	313	10	—
Sept.	1016	1943	113	—	258	315	—	15	—
Octob.	1477	1263	191	461	—	—	680	78	—
Nov.	1717	1107	91	240	—	—	156	—	100
Dec.	1790	1078	95	73	—	—	29	4	—
Jan.	1846	1006	104	56	—	—	72	9	—
Feb.	1976	948	81	130	—	—	58	—	23

THE following TABLE contains the Sum of the *Excreta* in the different Seasons.

[*To be added to* TAB. VI. *Transf.* N^o. 470. p. 508.]

	Urine.	Perspirat.	Stools.
Spring:	5393	3377	263
Summer.	3662	5139	266
Autumn.	3767	4834	402
Winter.	5353	3191	290

THE succeeding TABLE contains in Ounces and Centesimals the mean diurnal and nocturnal Urine and Perspiration of one Hour in each Month, with their *Ratio's* to each other: and that the Causes of the great Disproportion which they bear to each other, in the different Seasons, may be more conspicuous, I have added the mean thermometrical Altitude in the Heat of the Day, and at Bed time, in each Month of the Year.

Mean

	Mean Per- piration of one Hour.	Mean Urine of one Hour	The Ratio's which the diurnal and nocturnal Perspiration and Urine bear to each other.				Mean thermo- metrical Alti- tude.			The mean daily <i>Ingesta</i> .
	Nocturnal.	Diurnal.	The diurnal Perspiration is to the nocturnal as 1 to	The diurnal Urine is to the nocturnal as 1 to	The diurnal Perspiration is to the diurnal Urine as 1 to	The nocturn. Perspirat. to the noct. Urine as 1 to	At 3 in the Afternoon.	At Bed-time.	The Difference.	
March	1.85	1.71	0.924	0.826	1.71	1.52	63	54	9	117 38
April	2.45	1.72	0.702	0.766	1.10	1.19	74	65	9	109 03
May	2.75	1.77	0.644	1.164	0.81	1.46	78	70	8	117.92
June	3.42	2.1	0.611	1.117	0.58	1.21	83	74	9	125.51
July	4.42	2.08	0.471	1.510	0.35	1.14	86	76	10	138.78
August	3.6	1.66	0.461	1.400	0.57	1.73	81	74	7	130.42
September	3.63	1.77	0.488	1.260	0.46	1.20	78	72	6	118.22
October	1.74	1.68	0.966	1.374	0.98	1.40	63	56	7	96.89
November	1.78	1.57	0.882	0.832	1.57	1.48	56	50	6	109.62
December	1.98	1.42	0.717	0.764	1.61	1.70	48	41	7	118.43
January	1.88	1.37	0.728	0.467	1.94	1.24	50	43	7	118.76
February	1.74	1.36	0.782	0.571	2.24	1.63	52	46	6	118.46
The Means	2.61	1.68	0.644	0.922	0.98	1.40	68	60	8	117 88

From the preceding TABLE it appears, that,

1. The mean diurnal
Perspiration in the

$\left. \begin{array}{l} \text{Spring} \\ \text{Summer} \\ \text{Autumn} \\ \text{Winter} \\ \text{Year} \end{array} \right\}$	$\left. \begin{array}{l} .20 \\ .44 \\ .43 \\ .23 \\ .36 \end{array} \right\}$	$\left. \begin{array}{l} \text{Parts greater than the} \\ \text{mean nocturnal Per-} \\ \text{spiration in the} \end{array} \right\}$	$\left. \begin{array}{l} \text{Spring} \\ \text{Summer} \\ \text{Autumn} \\ \text{Winter} \\ \text{Year} \end{array} \right\}$	$\left. \begin{array}{l} \text{And the Days} \\ \text{warmer than} \\ \text{the Nights by} \end{array} \right\}$	$\left. \begin{array}{l} 8 \\ 9 \\ 7 \\ 6 \\ 9 \end{array} \right\}$	$\left. \begin{array}{l} \text{Degrees.} \end{array} \right\}$
--	--	---	--	--	--	--

2. The mean diurnal
Urine in the

$\left. \begin{array}{l} \text{Spring} \\ \text{Summer} \\ \text{Autumn} \\ \text{Winter} \\ \text{Year} \end{array} \right\}$	$\left. \begin{array}{l} .29 \\ .23 \\ .32 \\ .33 \\ .08 \end{array} \right\}$	$\left. \begin{array}{l} \text{Parts greater} \\ \text{Parts less} \\ \text{Parts less} \\ \text{Parts greater} \\ \text{Parts greater} \end{array} \right\}$	$\left. \begin{array}{l} \text{than the mean nocturnal Urine in} \\ \text{the} \end{array} \right\}$	$\left. \begin{array}{l} \text{Spring.} \\ \text{Summer.} \\ \text{Autumn.} \\ \text{Winter.} \\ \text{Year.} \end{array} \right\}$
--	--	---	--	---

3. The Excess of the mean diurnal
Perspiration above the nocturnal in

$\left. \begin{array}{l} \text{Spring} \\ \text{Summer} \\ \text{Autumn} \end{array} \right\}$	$\left. \begin{array}{l} \text{was} \\ \text{greater by} \\ \text{greater by} \end{array} \right\}$	$\left. \begin{array}{l} .04 \\ .72 \\ .67 \end{array} \right\}$	$\left. \begin{array}{l} \text{Parts, than was the Excess of the} \\ \text{mean diurnal Perspiration above the} \\ \text{nocturnal in Winter.} \end{array} \right\}$
--	---	--	--

4. The

4. The Excess of the mean diurnal Perspiration above the nocturnal, in *July*, *August*, and *September*, taken together, which are our sickly Months, exceeded the Excess of the mean diurnal Perspiration above the nocturnal in Winter, by .79 Parts of what was the mean diurnal Perspiration above the nocturnal in these 3 hot Months taken together.

5. Though the mean nocturnal Heat in *July*, *August*, and *September*, taken together, exceeded the mean diurnal Heat in *November*, *December*, *January*, and *February*, taken together, by 22 Degrees; yet the Perspiration, under such very different Degrees of Heat, was nearly equal. For the mean nocturnal Perspiration of one Hour, in these 3 warm Months taken together, was 884 Grains; and the mean diurnal Perspiration of one Hour, in these 4 cold Months taken together, was 889 Grains.

6. In *July*, tho' the Excess of the mean diurnal Heat above the nocturnal exceeded the Excess of the mean diurnal Heat above the nocturnal in Winter by 4 Degrees only; yet the Excess of the mean diurnal Perspiration above the nocturnal in *July* exceeded the Excess of the mean diurnal Perspiration above the nocturnal in Winter, by $\frac{82}{100}$ Parts of what was the Excess of the mean diurnal Perspiration above the nocturnal in *July*.

7. The Day and Night's Perspiration, at a *Medium*, in equal Spaces of Time, was more nearly equal in the cold than in the hot Months; and in *October* and *November*, or at the Accession of the cold Weather, they were more equal than in any Month of the Year, *March* excepted.

8. The Evenings and Nights are not absolutely colder in proportion to the Heat of the Day in Autumn,

tumn, than in any other Season of the Year, as *Celsus* has affirmed; but are only so relatively, as, at that Season, we are weakest, and most sensibly affected by any Increment or Decrement of the Air's Heat.

9. In *June, July, August, and September*, taken together, the Excess of the mean nocturnal Urine above the diurnal, in equal Times, was .65 Parts deficient of what was the Decrement of the nocturnal Perspiration, or the Excess of the diurnal Perspiration above the nocturnal in these 4 warm Months taken together.

10. In Summer, the Excess of the mean nocturnal Urine above the diurnal was .63 Parts less than was the Decrement of the nocturnal Perspiration in that Season.

11. In Autumn, the Excess of the mean nocturnal Urine above the diurnal was .34 Parts less than what was the Decrement of the nocturnal Perspiration in that Season.

12. The Excess of the mean nocturnal Urine above the diurnal was not equal to the Decrement of the nocturnal Perspiration in *May, June, July, August, September*, by .61, .58, .66, .58, .63 Parts of what was respectively the Decrement of the nocturnal Perspiration in these Months; but, in *October*, the Excess of the nocturnal Urine above the diurnal was .91 Parts greater than was the Decrement of the nocturnal Perspiration in that Month.

13. If the mean diurnal *Ingesta* had been 100 Ounces thro' the Year, and the Excretions proportionate from that Quantity to what they were from the real mean diurnal *Ingesta* of each Month, then, by Computation, it appears, that

April

A. The mean diurnal Perspiration in	May	was increased	.04	Parts of what was the mean diurnal Perspiration in	April	and was increased	.33	Parts of what was the mean diurnal Perspiration in March.
	June	was diminished.	.14		May		.42	
	July	was increased.	.18		June		.52	
	August	was increased.	.16		July		.43	
	Sept.	was diminished.	.10		August		.49	
	Octob.	was diminished.	.42		Sept.		.23	
	Nov.	was increased.	.10		Octob.		.47	
	Dec.	was diminished.	.10		Nov.		.52	
	Jan.	was increased.	.07		Dec.		.48	
	Febr.	was diminished.	.07		Jan.		.52	

B. The mean nocturnal Perspiration in	April	was increased	.08	Parts of what was the mean nocturnal Perspiration in	March	and was increased	.03	Parts of what was the mean nocturnal Perspiration in March.
	May	was diminished.	.05		April		.13	
	June	was increased.	.10		May		.06	
	July	was diminished.	.07		June		.12	
	August	was increased.	.18		July		.03	
	Sept.	was diminished.	.15		August		.16	
	Octob.	was increased.	.13		Sept.		.01	
	Nov.	was diminished.	.17		Octob.		.22	
	Dec.	was increased.	.22		Nov.		.21	
	Jan.	was equal to what it was in	.03		Dec.		.21	
	Febr.	was equal to what it was in	.03		January		.21	

April

C, The mean diurnal Urine in	April	was diminished.	.08	Parts of what was the mean diurnal Urine in March.	March	and was diminished	.38
	May		.32		April		.41
	June		.05		May		.57
	July		.26		June		.41
	August	was increased	.25		July	& was equal to the diurnal Urine in March.	.47
	Sept.	was diminished.	.09		August		.35
	Octob.	was increased	.19		Sept.		.06
	Nov.		.31		Octob.		.12
	Dec.		.06		Nov.		.18
	Jan.		.12		Dec.	and was increased	.12
	Febr.		.07		Jan.		.18

D, The mean nocturnal Urine in	April	was diminished.	.15	Parts of what was the mean nocturnal Urine in March.	March	and was diminished	.01
	May		.14		April		.09
	June		.07		May		.20
	July		.13		June		.01
	August	was increased	.20		July	and was increased	.19
	Sept.	was diminished.	.19		August		.09
	Octob.	was increased	.26		Sept.		.05
	Nov.	was diminished.	.13		Octob.		.08
	Dec.		.03		Nov.		.47
	Jan.		.12		Dec.		.16
	Feb.		.37		Jan.		

The following TABLE exhibits, in Ounces and Centesimals, the greatest and least mean diurnal Urine and Perspiration of one Hour.

[*This to be added to TAB. II. Transf. N°. 470. p. 503.*]

	Mean diurnal Urine of one Hour		Mean diurnal Perspi- ration of one Hour	
	greatest	least	greatest	least
March	5.01	1.81	3.64	1.07
April	4.41	1.47	3.37	1.42
May	3. 9	0.83	4.42	1.32
June	3.89	1.15	5.25	1. 7
July	3.88	0.71	5.64	2.36
August	3.52	0.95	5.56	1.85
September	3.95	0.62	5.12	2.22
October	3.46	0.67	2.66	0.93
November	4.83	1.14	2.11	1.36
December	5.13	1.60	3.04	1.34
January	6.39	1.40	2.61	1.45
February	6.29	2.05	2.26	0.79

A TABLE of the Depth of Rain in Inches and
millesimal Parts in *Charles-Town*.

	1733	1739	1740	1741	1742	1743	The Means
January	1. 097	2. 310	4. 873	4. 492	2. 189	3. 172	3. 022
February	4. 416	2. 875	3. 084	3. 535	1. 650	2. 435	2. 999
March	4. 532	5. 609	1. 141	5. 713	5. 203	0. 621	3. 803
April	1. 082	0. 195	1. 092	1. 308	0. 918	5. 292	1. 648
May	3. 127	5. 120	5. 612	4. 841	5. 898	2. 535	4. 522
June	1. 567	15. 839	4. 648	5. 538	3. 250	1. 903	5. 458
July	10. 660	5. 452	3. 013	3. 399	1. 252	7. 738	5. 252
August	4. 104	12. 211	7. 301	7. 144	7. 647	3. 767	7. 029
September	10. 792	4. 834	3. 200	6. 734	2. 895	4. 686	5. 524
October	1. 358	6. 593	1. 257	3. 399	0. 759	1. 672	2. 506
November	2. 656	1. 235	1. 848	2. 964	3. 388	3. 220	2. 552
December	3. 877	3. 689	2. 736	1. 919	0. 957	2. 706	2. 647
Total Depth	49. 268	65. 962	39. 805	50. 986	35. 826	39. 747	46. 932

XXI. *A Letter from Mr. Henry Baker, F. R. S. to the President, concerning an extraordinary large fossil Tooth of an Elephant.*

S I R,

Read March 28.
1745.

THE fossil Tooth I have now the Honour to lay before you, was sent me lately, from *Norwich*, by Mr. *William Arderon*. It seems to be a Grinder belonging to the Left Under-Jaw of a very large Elephant, as its own Size and Weight may shew: For the Circumference, measured by a String drawn round the Edge, is 3 Feet, wanting 1 Inch; in Length it measures 15 Inches; in Breadth, where widest, 7 Inches, in Thickness above 3; and its Weight is upwards of 11 Pounds.

On one Side it is convex, and on the other concave, with 16 Ridges and Furrows running on each Side transversely, and corresponding with the same Number of Eminencies on the grinding Edge, which appears furrowed like a Millstone. On the Bottom of the Part that lay within the Gum are several Cavities for the Insertion of the Nerves. The whole Tooth is almost intire, and seems very little, if at all, petresied; but, since its being exposed to the Air, several little Cracks appear. Other monstrous Bones were found with it, as I am informed; and particularly Thigh Bones, 6 Feet long, and as thick as the Thigh of a Man; all which belonged probably to the same Animal, and may be considered as farther Proofs of the Creature's enormous Size.

The

The Place where, and the Manner how, these Bones were discovered, are Particulars so well deserving Consideration, that I shall make no Apology for relating them.

A little Town, called *Munstey*, is situated close to the Sea-shore, on the North-east Coast of the County of *Norfolk*, where the Sea is bounded by exceeding high rocky Cliffs: Some whereof being gradually undermined by the continual Dashing of the Waves when the Tide comes in, great Pieces frequently tumble down upon the Shore: And by the tumbling down of one of these the above mentioned Bones and Grinder were discovered.

Here therefore can be little Reason for imagining (as I know some have done, when such-like Bones have been found in more inland Counties), that the *Romans* brought Elephants over hither; which, when dead, they buried deep in the Earth, to prevent their becoming offensive: For they could never think of burying such a Carcase in a rocky Cliff, close to, or perhaps over-hanging, the Sea.—But, on the contrary, this Discovery seems a convincing Demonstration, that the Earth has undergone some very extraordinary Alterations: For the Remains of Animals, of quite different Climates and Regions, and of Kinds, which, in the present Situation of the World, could never possibly come over hither, must either imply their having been placed here by Providence, originally, or, that this Island must, heretofore, have been contiguous to the Continent: But, since we find these Creatures in very hot Countries only, it is highly probable they were never placed here by Providence; unless we can suppose the Temperature
of

of our Climate, as to Heat and Cold, to have been greatly alter'd: And, without such a Supposition, it would be no less unreasonable to imagine they would wander hither from warmer Regions, though even all the Quarters of the Globe should have been contiguous.

What Changes have happened to our Earth, and how [they have been produced, no human Wisdom can possibly find out with any Certainty: But suppose only the Polar Points, or *Axis* thereof, to have been shifted at any time but a few Degrees, and its Centre of Gravity to have been alter'd (which some great Men have imagined not improbable), what Convulsions in Nature, what an universal Change in the Face of Things, must thereby have been occasioned! What Inundations, or Deluges of Water, bearing every thing before them! What Breaches in the Earth, what Hurricanes and Tempests, must have attended such an Event! For the Waters must have been roll'd along, till, by them, an Equipoise was produced. — In short, all Parts of the World would thereby acquire different Degrees of Heat and Cold than what they had before. Seas would be formed where Continents had been: Continents would be torn in sunder, or perhaps split into Islands. The antient Bed of the Sea would be changed into dry Land, and appear covered at first with Shells, and other marine Bodies; of which the Acid and nitrous Salts of the Air would, in a few Years, moulder away and turn to Dust those upon the Surface; but such as were buried deep would be preserved and remain for many Ages.

Such would probably have been the Fate of inanimate Things: And as to living Creatures, they must have

have been almost universally destroyed and buried in the Ruins of the World, as perhaps this Elephant may have been. Some few, however, would in all likelihood escape, either by swimming to, or being left on, rising Lands; where, if they met with proper Food, and an agreeable Climate, they would continue and increase, or otherwise would wander till they found such a Country, unless prevented by interposing Seas, or impassable Rivers.

All this indeed is barely Conjecture: But the Bones and Teeth of Fishes, the Multitudes of Sea-Shells (some whereof are petrefied, and others not), and the many Sea-Productions found buried in the Earth in almost every Country, at vast Distances from the Sea, and even in the midland Parts, are Demonstrations of the surprising Alterations that must have happened as to the Disposition of Sea and Land. The Horns of the great Mouse-Deer, dug frequently out of the Bogs in *Ireland*, and sometimes in *England*, the Bones and Teeth of Elephants found there, and this present Discovery, together with some others of the like Kind that have been made in *England*, seem to prove, that such Animals formerly inhabited these Countries, notwithstanding the Mouse-Deer is known at present only in *America*, and Elephants are not found except in *Africa* and *Asia*.

Part of the Horn and Palm of a Deer, found in a Chalk-Pit, at a Village called *Baber*, 4 Miles East of *Norwich*, at the Depth of 16 Feet, and almost converted into a chalky Substance, being of a Kind of which, I am told, we have none in this Island, I also lay before you, as another Proof to the same Purpose.

Hoping

Hoping Pardon for this Digression, I shall just beg Leave to observe, that the present Grinder and Bones, however they came thither, must have lain in this Cliff for many Ages; and that the Grinder in particular is very much larger and heavier than any our late worthy President Sir *Hans Sloane* has mentioned in N^o. 403. and 404. of the *Philosophical Transactions*, where he gives an Account of all the fossil Teeth of Elephants that had come to his Knowledge. I may add, that none of those mentioned by Mr. *Molineux*, in his History of *Ireland*, come near it in Weight or Size. Our Thigh-Bones of 6 Feet long exceed also by 2 Feet any I have ever yet heard of: And, according to Mr. *Blair's* Osteology of an Elephant 9 Feet high, which died at *Dundee* in *Scotland*, in the Year 1706, and whose Thigh-Bones were 3 Feet in Length (*Vide Phil. Transf.* N^o. 327.), we may suppose, by the Rules of Proportion, that the Elephant, to whom our Bones and Tooth belonged, was 18 Feet in Height.

Permit me, Sir, to assure you, that I am, with the utmost Respect and Sincerity,

Your most obedient humble Servant,

London, March 26.

1745.

H. Baker.

See TAB. I. Fig. 7.

XXII. *An Account of an extra-uterine Conception ; by Starkey Myddelton, M. D.*

London, March 28. 1745.

*Read March 28.
1745.*

ALTHO' extra-uterine Conceptions have frequently appear'd from Cases of undoubted Authority, many of which stand now upon the Records of the *Royal Society* ; nevertheless I have thought a Doctrine of so extraordinary a Nature cannot be too strongly supported, as it is of the highest Consequence, as well in establishing the received Opinions of Conception in general ; as in regulating the Judgment and Practice of those who are more particularly employed in the Business of Midwifry.

I shall therefore make no Apology for laying before this Learned *Society* a Case which affords such convincing Proofs of this Doctrine, and comes at the same time too well attested to admit of any Doubt, either as to the Fact, or the Circumstances.

On the 28th of *October* last, I was sent for to a Woman of about 42 Years of Age. When I came, I was told by the Patient, that she had been taken with a Flooding the Day before ; which a little surpris'd her, as having been very irregular in her menstrual Discharges for near a Year before.

At the same time she complain'd, of a great Pain in her Belly and Loins, with a continual Forcing both forward and backward ; which still continued, tho' her Flooding was then in a manner stopp'd.

I order'd her a gentle Paregoric for that Night, and the next Day I found her in great Pain ; at which

time

time she told me, she had some Reason to believe she had conceived with Child.

I then examined her, and found the *Os Tincæ* intirely close. I was not very curious in my Examination at this time; taking it for granted, from what she had told me, that Nature would, in a little time, dispose the *Uterus* to discharge its Contents, tho' at present there was not the least Appearance of it. I order'd her an anodyne Clyster, and a Paregoric to be taken after it; and the next Day I found her Pains continued, which now appeared like a *Tenesmus*; tho' so violent, as to prevent her Rest all that Night. I then ordered her a Repetition of the Clyster and Paregoric; and the next Day (finding her in great Pain, and still without any Rest, and beginning to be a little feverish) I ordered 8 Ounces of Blood to be taken away, and continued the Paregoric, which I likewise order'd her to repeat as she found Occasion; from the Use of which she sometimes rested tolerably well; but as the Power of the Opiate went off, her Pains always returned.

Several Days having now passed without any Alteration, I again examined her, and found the *Os Tincæ* still as close as ever; but, on a stricter Inquiry, I felt something, which seem'd to me to be the Head of a Child fluctuating in its Membranes.

I told her my Thoughts of her Case, and that it was not in my Power to help her; but that Nature must take its Course, or at least point out a Method how to act. She seem'd very much surpris'd at my Opinion, and asked me, If ever I had met with such a Case before? I told her, I had been engaged in the Practice of Midwifry upwards of 20 Years; in all

which time I had never met with a Case of the like Nature: Being positive I felt the Head of a Child, but could not absolutely determine whether it was in the *Uterus* or *extra Uterum*.

I then told her, I would desire the Favour of Dr. *Bamber* to give her a Visit; which I did; and the next Day we went together; when, on Examination, he confirm'd what I had before asserted; but seem'd more inclinable to believe the Child (the Head of which he felt) was *extra Uterum*. Indeed he proceeded in his Inquiry at that time further than I had done before, having pass'd his Finger into the *Anus*, where, he said, he could distinguish the Head more plainly. We then both left her, after having order'd her to repeat the Purgative, when in more Pain than ordinary, and once in two or three Days to take a gentle lenitive Purge to keep her Body soluble; because the continual Use of Opiates would naturally tie her up.

In this manner she went on for about three Weeks longer; when I waited on Dr. *Nichols*, and desired the same Favour of him as I had before ask'd of Dr. *Bamber*, that I might have his Opinion also of a Case which appeared so very singular.

The next Day we went together; and when we came, I desired he would examine her, which he did; and after having heard all her Complaints, said, He was of Opinion, that there was some Abscess forming in (or in Contact with) the *Uterus*, which, very likely, in a little time, would break and discharge itself; but as, at that time, nothing of a Child could be perceived by the Touch, so he was obliged
to

to submit that to the Credit of my Opinion, who had before frequently felt it.

Thus she continued for about a Fortnight after this Visit; when, calling on her one Day, she told me, she was much easier than she had been; and that something came constantly draining away by the *Anus*, of a very offensive Smell, which, upon Examination, appeared to be true *Pus*. I now began to think Dr. *Nichols's* Opinion of her Case the most eligible, and the rather, as it was not inconsistent with my own Sentiments, that there had been a Child; which, being now dead, might have given Occasion for the forming such an Abscess.

In this State of violent Pain she continued to the Time of her Death, which happen'd on the 28th of *January*, being thirteen Weeks from the first of her Illness; when, by her particular Desire, I open'd her, in the Presence of Dr. *Bamber*, Dr. *Nicholls*, Dr. *Eaton*, and Mr. *Jones* Surgeon, &c.

After having divided the Integuments of the *Abdomen*, every thing, at first View, appeared in a healthy State. On turning aside the Intestines, I found the *Uterus* sound and perfect, and of a Size common to Women who have had Children; but, in the Place of the Right *Fallopian* Tube, there appeared a large Tumour, formed by the Expansion of the Tube extending itself from the *Os Ilium* towards the Extremity of the *Sacrum*. Upon opening it, we discover'd a Mass of fetid *Pus*, in which the Bones of a *Fœtus*, of about 5 or 6 Months old, were buried. These Bones were, for the most part, wholly divested of their Flesh; so that the Edges of the thin Bones must, of Necessity, cut and irritate from every Motion
of

of the Body. The *Pus* had made its Way thro' the *Rectum*, in which there was a small Passage a little above the *Sphincter*.

Upon examining the Bones, after having wash'd them in Water, a new Matter of Surprize appeared; viz. the inferior Jaw was consolidated with the *Os Temporis* and superior *Maxilla*; and six of the Ribs, with their correspondent *Vertebrae*, were united into one Bone.

May we not attribute these *Anchyloses* to the Want of Motion in the *Fœtus*, which was here prevented, by its being closely pent up in so unnatural a Situation? If this be the Case, we see great Advantages arising from the frequent Motions of the *Fœtus* in the *Uterus*; and that Providence has not subjected the tender Part of our Species to such constant Disturbances without great Necessity.

See TAB. I. Fig. 8.

A, The *Uterus*.

B, The *Os Tincæ*.

C, The *Vagina*.

D, The left *Fallopian* Tube.

E, The Beginning of the right *Fallopian* Tube in its natural State.

F, The *Sacculus*, formed by the Expansion of the right *Fallopian* Tube, in which the *Fœtus* was contained.

G, The *Ligamentum rotundum* on the left Side.

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- I. *Two Letters from the Rev. Mr. Granville Wheler, F. R. S. to the President, concerning a rotatory Motion of Glass Tubes about their Axes, when placed in a certain Manner before the Fire.*

I.

S I R,

London, Mar. 28. 1745.

Read on March
28. and April 4.
1745.

ABOUT four Years ago, Mr. Charles Orme, of *Asbby de la Zouch* in *Leicestershire*, acquainted me, That, in drying his glass Tubes for his diagonal Barometers (which for some Years he has continued to make in much greater Perfection than any other Person that I know of in *England* *), he had observed a rotatory Motion about their *Axes*, and at the same time a progressive one towards the Fire. He was so obliging then as to promise at any time to shew me the Experiment; but other Business intervening, I still deferred accepting his Offer; having the less Curiosity to see it, as I imagined the Motions were occasioned by the Draught of Air up the Chimney, assisted by the Weight of the inclining Tube. But a little above a Year ago, making some Stay at *Asbby*, upon repeating his Offer, I went to see the Experiment, which answered fully to his Description: The Tubes, which were about four Feet long, and half an Inch over, moving at six or eight Inches Distance from the Fire, not only progressively, and about their *Axes* along the Side-Wall they lean'd against, but along the Front-Wall

X x
of

One of which Mr. *Wheler* made a Present of to the *Royal Society*.

of the Chimney, which made an obtuse Angle with the other ; so that they seemed to move up-hill, and against their Weight.

Surprised at this, I thought the Case deserved a little farther Examination ; and proposed placing two Tubes horizontally, parallel to each other, and at right Angles to the Face of the Fire, to be Supporters to a third, which was to be placed upon them parallel to the Fire. We did so, and with Pleasure observed the supported Tube turn about its *Axis*, and move on towards the Fire in such a manner, as made me still less inclined to think either of the Motions owing to the Draught of the Fire, and certainly not to the whole Weight of the moving Tube ; a fine Spirit-Level informing us, that the supporting Tubes lean'd from the Fire ; so that the Motion was a little up-hill.

This Success determined me, with Mr. *Orme's* Leave, to go on farther ; and, furnishing myself, from him, with Tubes of several Lengths and Thicknesses, I made several Trials ; and found, that with a moderate Fire the Experiment succeeded best, when the supported Tube was about twenty or two-and-twenty Inches long, the Diameter about one Tenth of an Inch, and had in each End a pretty strong Pin, fixed in Cork, for an Axe to roll with upon the supporting Tubes ; which, to lessen the Contact, had nearly the same Diameter with the moving one. Under these Circumstances the Tube would begin to move at eighteen Inches Distance from the Fire ; and continue to do so, with little Intervals, till it touched the Bars ; and moved much in the same manner, when a little Ball of Cork, an Inch or more in Diameter,

meter, was fixed in the Middle of it. But what surprised me still more, and seemed to take off the Objection of the Draught of the Chimney, was, letting it once stay a little while against the Bars, I found it still continue its Motion about its *Axis* in the same Direction.

This put me upon making little Rings of Wire, to fix upon and move along the supporting Tubes, so as to stop the moving Tube at any Distance from the Fire I pleased.

Stopp'd with these, the Motion of the Tube about its *Axis* still continued.

Desirous to try what would be the Effect in or near an upright Posture, I made the Pin at one End of my Tube rest upon a *China* Plate, that at the other Turn in a silver Socket (that carried my Pencil) fixed in an horizontal Arm of Wood, but so as I could slip it up and down, to adapt it to the Length of of the Tube. Here I found, that if the Tube lean'd to my Right hand, (which was the Case of Mr. Orme's Tubes before his Fire) the Motion was from East to West; but if they leaned to my Left, the Motion was from West to East; and the nearer I could get to the perfectly upright Posture, the less the Motion seem'd to be either Way.

I now proceeded to place my Tube horizontally upon a glass Plane (a large Fragment of a Coach-side Window Glass). The Tube, instead of moving towards the Fire, moved from it, and about its *Axis*, in a contrary Direction to what it had done before, Observing that this glass Plane was broader at one End than the other, and that the Rotation backwards was more sensible when the narrower End was

towards the Fire, I placed a triangular Piece of the same Glass with its *Vertex* towards the Fire nearly horizontal, but rather rising from the Fire; so that its Base was a little higher than its *Vertex*; and upon it a Tube of Glass, about 22 Inches long, and $\frac{1}{8}$ of an Inch Diameter, near the *Vertex* and the Fire. This Tube receded from the Fire, moving about its *Axis* till it came to the Distance of eight Inches; which is four Inches more than it receded the Day before upon the same Piece of Coach-Glass, before it was broke into this triangular Form.

I was naturally led now to make use of two supporting Tubes, instead of the triangular glass Plane. These were about eighteen Inches long each, and $\frac{1}{10}$ of an Inch in Diameter, and placed parallel to one another at the Distance of about two Inches, so as to support the moving Tube near the Middle of it. When very nearly horizontal by the Level, the supported Tube moved from the Fire about its *Axis* to the Distance of thirteen Inches: When the Supporters were a little raised at their remote Ends, so as manifestly by the Level to descend towards the Fire, it receded to the Distance of ten Inches, moving as before about its *Axis*; but in this latter Case the Fire had declined a good deal; otherwise, probably, the Tube would have receded farther, tho' up-hill.

The next Day, the same Tube, when the same supporting Tubes were $8\frac{1}{2}$ Inches distant from each other, receded nearly as before: When $12\frac{1}{2}$ Inches from each other, it stood still; and when removed to the Distance of $16\frac{1}{2}$ Inches, the supported Tube very manifestly changed its Motion, and went towards
the

the Fire; as it did afterwards, when the Inclination of the supporting Tubes was alter'd, so as to ascend towards the Fire.

I made several other Experiments, with regard to the Situation of the Tubes to the Fire, with regard to the Quantity of Fire suffered to come at the Tubes, and with regard to Attraction and Repulsion, which I will not trouble you with at present: Only observe, that, when the Tube had four others under it, all supporting, one near each Extremity, and one on each Side of its Centre, no Motion at all was perceived; and when two of them on the same Side of the Centre were taken away, the supported Tube moved into an oblique Situation with regard to the Fire, the unsupported Half receding from the Fire.

Upon the Whole, it appears sufficiently plain, that the Stream of Air up the Chimney is not the Cause of the Rotation: Another may be assigned, simple and easy; but as I have already said too much, it will be better to make it the Subject of another Paper. I am,

S I R,

Your most Obedient

Humble Servant,

Granville Wheler.

II.

SIR,

IN the last Paper I had the Honour of communicating to you, I endeavoured to make it appear, that the rotatory Motion of glass Tubes about their *Axes*, before a Fire, was not owing to the Draught of the Chimney. In this, I beg Leave first to mention an Experiment or two, to shew that the Motion is not owing to any Attraction or Repulsion in the Tubes; and then give the Solution I proposed in my last, but deferred laying before you, because my Paper was already carried to too great a Length.

I suspended two Fragments of small Tubes, 8 Inches long, and about $\frac{1}{10}$ of an Inch in Diameter, near the Fire, from two Pins, by blue Silk Lines, which had each a Loop at one End, were tied at the other to the Top of the Tubes, and hindered from slipping off by a little Sealing-wax. The Tubes came together at the upper End, and receded manifestly from each other at the lower, appearing to be in a State of Attraction above, and a State of Repulsion below: But, suspecting this to be owing to the Sealing-wax, which soon began to melt, I scraped it off both, leaving only as little as was possible, to hinder the Silks from slipping. The Consequence then was, they came together at the lower Ends, and very near so at the upper; and, when suspended from one Pin, so that the Loops of the Silks touch'd each other, the Tubes seem'd equally close all the Way down, without any Appearance either of Attraction or Repulsion. But, imagining still that a repulsive Power

Power in the heated supporting Tubes, when placed near together, might possibly be the Occasion of the receding of the upper Tube at Contact with them. To put the Matter out of all Doubt, I wet the three Tubes all over; yet the regressive and rotatory Motion was still manifest, with very little, if any Difference; not more than might be well accounted for, from the Increase of Resistance by Wetting.

These two Experiments fully convinced me, that neither Attraction nor Repulsion would be of any Assistance in solving our Rotation. Upon considering therefore the Matter farther, I found nothing was wanting, but that the moving Tube should swell towards the Fire; and indeed I thought I could perceive such a Swelling in Mr. *Orme's* long Tube of four Feet and an half, which I saw first placed near a good Fire in the Manner described in my last. For, allowing such a Swelling, Gravity must pull the Tube down, when supported near its Extremities horizontally; and a fresh Part being exposed to the Fire, and swelling out again, must fall down again, and so on successively; which is, in other Words, a rotatory Motion towards the Fire.

When the supporting Tubes are brought near to each other, as well as near to the Centre of the supported Tube, then the Parts hanging over on each Side, being larger than the Part which lies between the Supporters, will, by their Weight, pull downwards, and consequently force the middle Part, resting upon its two *Fulcra*, upwards; and being less advanced towards the Fire, as being less heated, will, by their oblique Situation, pull the middle Part backward also from the Fire: Which Effects, being successive,

cessive, will exhibit a rotatory regressive Motion, quite contrary to what the Tube had when supported near its Extremities: And when a single Tube lies inclining opposite to the Fire, either to the Right-hand or the Left, out of a Plane perpendicular to the Surface of the Fire, Gravity will not permit the curved Part to rest, but pull it down till it co incides with a Plane perpendicular to the Horizon; and, consequently, as new Curves are generated, new Motions will be so too; that is, the Tube will be made to move about its *Axis*; but with this Difference, when the Tube inclines to the Right hand, the Motion about the *Axis* will be from East to West; when to the Left-hand, from West to East. The Justness of this Reasoning is made manifest with a very little Trouble; only bending a Wire, and supporting it first near its Extremities, then near its Centre on each Side, afterwards inclining it to the Right, and then to the Left; the Bending in every Case representing the curved Part of the Tube next the Fire. And that this Solution is the true one, seems farther probable from hence, that when four Supporters were made use of, one at each Extremity, and two near the Middle, there was no Motion at all either backward and forward: Nor is it of any Service to object here, that the Increase of Contact hinders the Motion; because, upon the Plane of Glass, mentioned in my former Paper, so large as to have a much greater Contact with the Tube, both a rotatory and regressive Motion was manifest. I am, worthy Sir, with a high Regard,

Your most obedient humble Servant,

London, April 4. 1745.

Granv. Wheler.

II.

II. *An Attempt by John Ward, Rhet. Prof. Greth. & F. R. S. to explain some Remains of Antiquity lately found in Hertfordshire; and communicated to the Royal Society by William Freeman Esquire, one of their Members, February the 14, 1745.*

Read April 4. 1745. **B**Y the Account, which that Gentleman delivered in with them, they are said to have been found in a Chalk Pit, near the Side of *Rooky Wood*, in the Parish of *Barkway* in *Hertfordshire*. A Farmer's Man digging Chalk there about two Years since brought them with the Chalk into his Master's Yard, and taking no Notice of them mixed them with the Dung. But very lately on throwing up the Dung in that Yard they were discovered by Mr. *Raymond*, Steward to *George Jennings* Esquire, Lord of the Manor. Upon which at the Desire of *Pulter Forester* Esquire, an ingenious Gentleman in that Neighbourhood, they were shewn to him; who had the Curiosity to delineate those two Plates, which have Inscriptions upon them, and sent the Draughts to Mr. *Freeman*. And he soon after receiving the Originals from Mr. *Jennings* laid them, together with the two Drawings, before this Society; when the Form of the Plates, and Novelty of the Work, occasioning the Assembly then present to think they might deserve some further Consideration, it was their Pleasure to desire me to give them my Opinion, both as to the Reading, and Design of them. This I have indeavoured to do, in the best Manner I

Y^y could;

could; and now beg Leave to offer, what appears to me most probable, on a Subject so intirely new.

Barkway lies not far from *Royston* in the same County, by which the *Roman Way* called *Erming-street* passes to *Huntington*, as described by *Camden* (a). But as no *Roman Station* has been discovered near either of those Towns, it may be difficult to assign a Reason, how these Things should come to be lodged in the Place, where they were found; tho several Instances of the like Nature have happened in diverse other Parts of this Kingdom. They consist of a small brass Image, an oblong Peice of Brass, and seven silver Plates very thin; which have all suffered more or less by Time, and other Accidents. The Figures and Ornaments on the Front of the Plates are all in Relief, and seem to have been made by a Stamp impressed on the other Side. Two of them have Inscriptions in a Compartment, written with the Point of a Style, and the Letters flatted behind. And one of these with three others of the remaining five have the Figure of *Mars*, and the other two that of *Vulcan*, impressed upon them.

The Image represents *Mars* (see TAB. I. Fig. A.) looking to his right Side, with an Helmet on his Head, and his Body naked; his right Arm extended, as if he had held a Spear, and a Thong round his left, like the Remains of a Sheild now broken off with the Hand; his right Shoulder supporting a Belt, which crossing his Body descends on the left Side; his right Leg broken off at the Knee, and his left Foot lost. The
Hight

(a) *Britann.* pag. 356, edit. 1607.

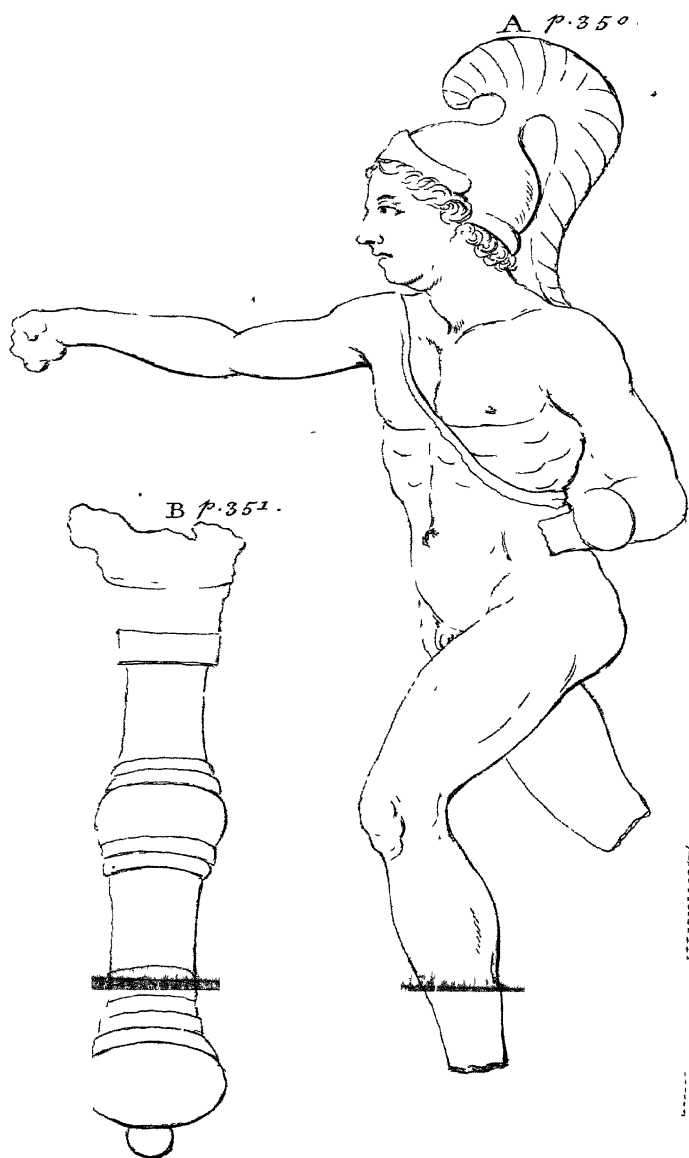
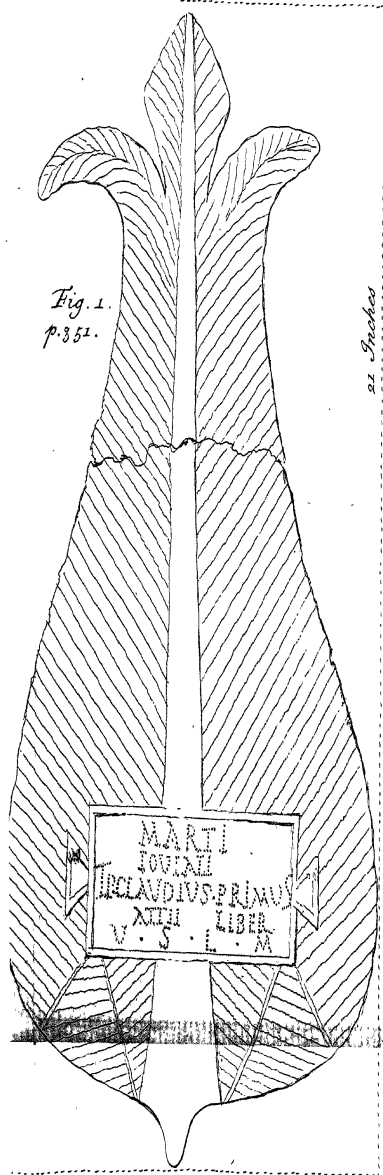


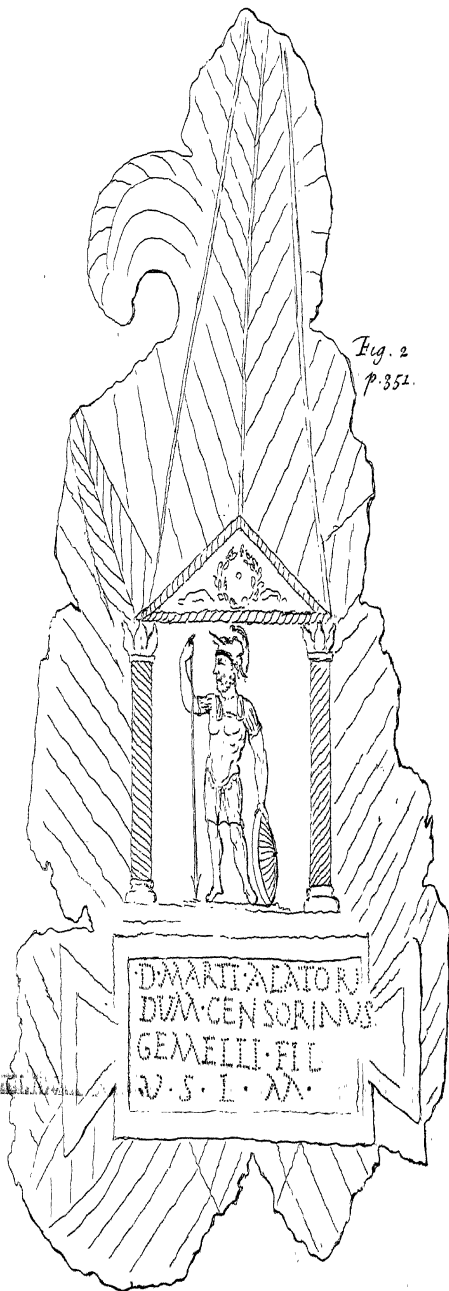
Fig. 1.
p. 351.



21 Inches

10 Inches

Fig. 2
p. 351.



Hight of what remains, from the End of the left Leg to the Top of the Crest, is about seven Inches. It has been observed by *Montfaucon*, that tho *Mars* is frequently represented on Coins, yet his Statues are not very common (a).

The other Peice of Brass (*see* TAB. I. *Fig.* B.) is about four Inches and a half long, and seems to have been the Handle of a Knife, or some such Utensil.

The Plates are for Distinction sake numbered in the following Order.

1. The first is broken into two Parts, (*see* TAB. I. *Fig.* 1.) which put together resemble the Form of a Leaf, and is near twenty one Inches high, and about ten broad in the widest Part. It contains an Inscription inclosed in a Compartment, addressed to *Mars Jovialis*, which will presently be explained.

2. The second (*see* TAB. I. *Fig.* 2.) is eight Inches in Hight, and four in Breadth, where it is widest; and retains pretty much of the Gilding, which none of the rest now do: tho all of them it seems, when first discovered, appeared to have been gilt, but in washing them the Gilding came off. It has on it an Image of *Mars* in a military Habit, with an Helmet on his Head, a Spear in his right Hand, and his left resting on a Sheild; in the Manner he is often seen upon Coins. This Image is placed in the Front of a Temple, between two Pillars, with a *Fastigium* or Pediment over them. And beneath the Temple in a Compartment is an Inscription to *Mars Alatorum*, which I shall endeavour to explain afterwards.

3. In

(a) *Supplem.* Vol. I. p. 93.

3. In the third Plate, (*see* TAB. II. *Fig.* 3.) which is three Inches high, and almost two wide, is an Image of *Mars* placed between two Pillars without a Pediment, in much the same Attitude as the former, with a *Parazonium* over his Shield.

4. The fourth Plate, (*see* TAB. II. *Fig.* 4.) which is four Inches three Quarters in Height, and one Inch three Quarters in Breadth, has the Figure of *Mars* in a like Attitude; inclosed only in a plain Compartment.

5. The Height of the fifth (*see* TAB. II. *Fig.* 5.) is eight Inches, and the Breadth near four and a half; which has also a Figure of *Mars*, much like those already mentioned, but turning to the left Hand (whereas they all turn to the right) with a *Chlamys* hanging down on his right Side. It stands in the Front of a Temple, having two Pillars on each Side, called by *Vitruvius tetrastylos* (a), and a double Pediment over them.

6. The sixth Plate (*see* TAB. II. *Fig.* 6.) is six Inches three Quarters in Height, and three and a Quarter in Breadth. It differs from all the former, as it represents the Figure of *Vulcan*, having his usual Attributes, a thick Beard, high Cap, short Tunic, *Femoralia*, and half Boots; a *Forceps* in his right Hand, and a Hammer in his left, with a *Chlamys* thrown over his left Arm. He looks to the right, and has before him a Vessel like an Altar, from which a Flame ascends. He is placed in the Front of a Temple, between two Pillars under a Pediment, like *Mars*.

7. The last Plate (*see* TAB. II. *Fig.* 7.) is three Inches and a half high, and near two Inches wide. It contains

Fig. 3.

p. 352.



Fig. 5.

p. 352.

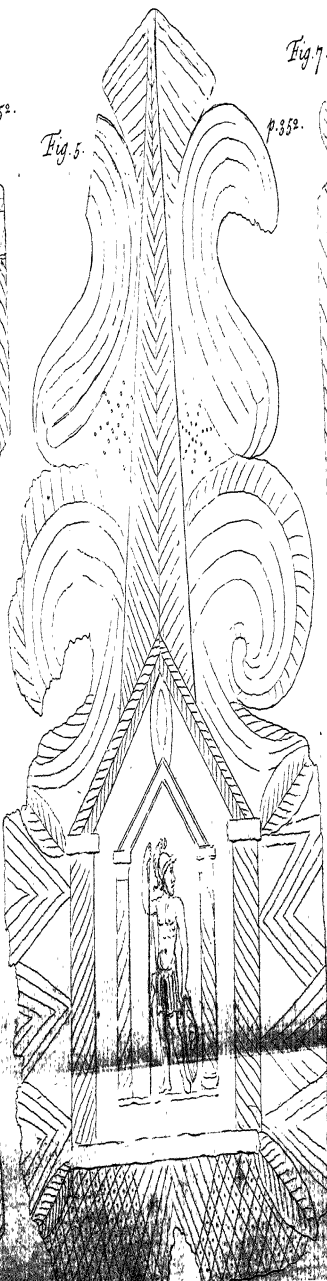


Fig. 7.

p. 352.



Fig. 4.

p. 352.

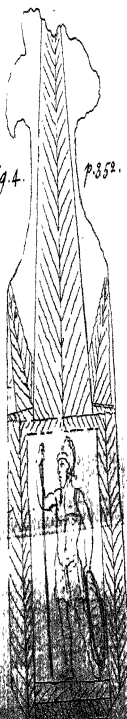
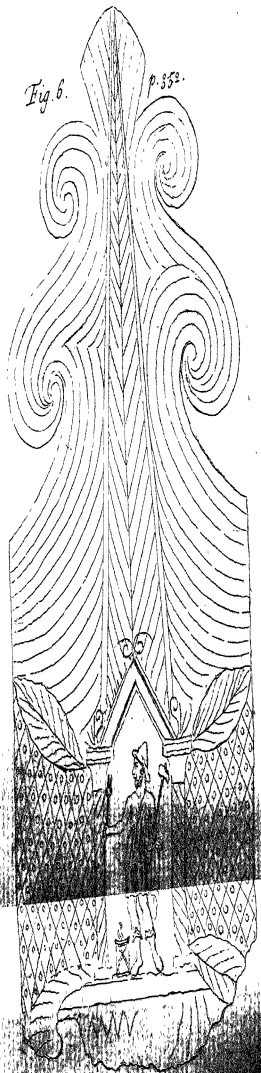


Fig. 6.

p. 352.



contains also an Image of *Vulcan* in the Front of a Temple, with his several Attributes, like the former, but differs from it in the other Ornaments.

The Design of both the Inscriptions is to return Thanks for some Favour ascribed to the Deity, to whom they are addressed. That on the first Plate runs thus :

MARTI
IOVIALI
TI. CLAUDIVS . PRIMVS
ATTII . LIBER
V . S . L . M

That is

*Marti Joviali Titus Claudius Primus, Attii libertus,
votum solvit libens merito.*

The Word IOVIALI, in the second Line, seems to be an Epithet given to *Mars* in Compliment to the Emperor *Diocletian*, who assumed the Name of *Jovius*; as his Colleague *Maximian* did that of *Herculius*. Hence we meet with some military Bodies in the *Notitia*, and elsewhere, called *Joviani* and *Herculiani* from those Emperors; like the *Flaviani*, *Aeliani*, and the like, which were so denominated from the Names of other preceding Princes. There are also other Epithets of the same Form with that in the Inscription, taken either from the Names of Deities, or Emperors deified; such were the *sacerdotes Augustales, Flaviales, Hadrianales*, and others, which often occur in *Gruter*. In like manner *Cicero* gives the Title of *ministri Martiales* to the Priests of *Mars*

Mars (a); and calls the Company of Merchants at *Rome Mercuriales* (b), as being under the Protection of *Mercury*. And *Ganymedes* is stiled by *Macrobius*, *Jovialium poculorum minister* (c). Now as these several Appellations took their Rise from the peculiar Relation and Subserviency of the Persons to those Deities, from whom they were denominated; so *Mars* himself, being here called *Jovialis*, is by an Excess of Flattery, represented as subservient to this Emperor *Jovius* or *Jupiter*. For so he was also called, as we find in some like Instances of fulsome Compliments paid to him by the Panegyrist *Mamertinus*; as when addressing to him, and his Collegue *Maximian*, he saies: *Sancte Jupiter et Hercules bone* (d). And in another Passage: *Non opinione traditus, sed conspicuus et praesens, Jupiter cominus invocari; non advena, sed imperator, Hercules adorari* (e). And as if no Degree of Flattery could be too extravagant for this Emperor, there is an Inscription in *Gruter*, which begins thus: AETERNO IMPERATORI NOSTRO MAXIMO OPTIMOQUE PRINCIPI AVRELIO VALERIO DIOCLETIANO (f). The Epithets OPTIMVS MAXIMVS, usually ascribed to *Jupiter*, had indeed been applied to some former Emperors; but AETERNVS, as a personal Title, seems to have been first attributed to this Prince; tho, like other ill Examples,

(a) *Pro Cluent.* cap. 15.(b) *Ad 2. Fr. Lib. II. Ep. 5.*(c) *Saturnal. Lib. V. cap. 16.*(d) *Genethliac. Maxim. cap. 16.*(e) *Ibid. cap. 10.*(f) *Pag. ccxxxix. 4.*

ples, it was soon imitated, and given to some following Emperors.

The third Line contains the Names of this Votary, TITVS CLAVDIVS PRIMVS, each of which is separately found in *Horsley's Britannia Romana*; and in one of *Gruter's* Inscriptions they all three meet in the same Person, in the Order they stand here (a). The next Line tells us his Character, that he was the Freedman of ATTIVS, that is, as I suppose, of TITVS CLAVDIVS ATTIVS; it being customary for Freedmen to assume the two first Names of their Patrons, as TIRO the Freedman of *Cicero* was called MARCVS TVLLIVS TIRO. Indeed ATTIVS generally stands as a Family Name, but we find it in the Place of a *Cognomen* in *Gruter*, MARCVS TVLLIVS M. L. ATTIVS (b). The last Line contains the usual Form of such Addresses.

The Inscription on the second Plate is thus expressed:

D . MARTI . ALATOR
DVM . CENSORINVS
GEMELLI . FIL
V . S . L . M

That is, as I apprehend it may be read :
*Deo Marti Alatorum Dum. Censorinus, Gemelli filius,
votum solvit libens merito*

The Word ALATORV in the first Line must, I think, stand for ALATORVM, the Letter V being joined to the R in one Character; as we find them
in

in the *Britannia Romana*, where they make Part of the Word *INSTITVERNT* for *INSTITVERVNT* (*a*). How frequent and various such Combinations were, especially under the lower Empire, appears by the *Table* of them published in that Work (*b*). Some of which seem to have been the Effect of Fancy in the Workmen, and others occasioned thro Want of Room, as in the present Case. As to the Meaning of the Word *ALATORVM*, I suppose it to be an Adjective, the Substantive *CASTRORVM* being understood; and that the same Place is intended, which *Ptolemy* calls *Πτερωτόν τεγαμένον* (*c*), and modern Geographers generally take for *Edinburgh*. For as *Ptolemy* was himself a Stranger to that Country, his *Greek* Name was probably an Interpretation of the *Latin*, *Castra Alata*; which Mr. *Horsley* thinks might be so called from the Situation of the Place somewhat resembling a Wing (*d*). But as there is good Reason to think, that this Inscription was written long after the Time of *Ptolemy* (as will be shewn afterwards) the Word *CASTR*A might then have been dropt, and the common Appellation of the Place be only *ALATA*. There are other Examples of the like kind, which may render this very probable. For we meet with a *Roman* Station in the County of *Essex*, which both in *Antonine's Itinerary of Britain* (*e*) and *Peutinger's Tables* is called *AD ANSAM*, from the angular Turn of the Road there, as it is laid down in

(*a*) Northumb. xv. (*b*) Pag. 189. (*c*) *Ibid.* pag. 359.
 (*d*) *Ibid.* pag. 364. (*e*) *Iter* ix. *Ibid.* pag. 381.

in the *Table of Britain* (a). There was another in *Nottinghamshire*, which the *Itinerary* calls AD PONTEM (b), on the Account of a Bridge laid over the *Trent* at that Place. And what appears more exactly parallel with the present Case, in the same *Itinerary* we meet with MAGNA (c), which some have taken for *Old Radnor*; but Mr. *Horsley* fixes it at *Kenchester* in *Herefordshire*, wherein he is followed by *Wesselingius* (d). Now in each of these Instances CASTRA, or some equivalent Word, must necessarily be understood; and probably at first the usual Names were CASTRA AD ANSAM, CASTRA AD PONTEM, and CASTRA MAGNA, tho afterwards the Word CASTRA was for Brevity omitted; as in common Speech we often find the Names of Places so shortened, that it is difficult to trace them back to their Original. And this Inscription might be addressed to DEO MARTI ALATORVM, as the topical Deity of the Place.

The second Line, DVM CENSORINVS, seems to contain two Names of the Person, who paid this Vow to the Deity here mentioned. Tho *Roman* Citizens had usually three Names, called *Prænomen*, *Nomen*, and *Cognomen*; yet very often two only are expressed: and those either the first and second, as *Quintus Horatius* [Flaccus]; the first and third, as *Marcus* [Portius] *Cato*; or the second and third, as [Caius] *Cornelius Tacitus*. Besides, the Order of these several Names did not always continue the same,

(a) *Ibid.* Pag. 505.
XII. *Ibid.* pag. 457.

(b) *Lit. VI. Ibid.* pag. 381.
(d) *Vet. Rom. Itiner.* p. 485.

(c) *Iter*

same; but what was at first a *Praenomen*, became afterwards a *Cognomen*: as PRIMVS in the former Inscription. And the *Cognomen* often became hereditary, and distinguished different Branches of the same Family; which I take to be the Case here. One of *Horace's* Odes in some Editions is inscribed to *C. Marcius Censorinus* (a); but *Censorinus* often stands as a Family Name in *Gruter*, and others. So likewise in this Inscription DVM. CENSORINVS is said to be FILIVS GEMELLI, that is GEMELLI CENSORINI. But as I do not find any other Instance of a *Roman* Name beginning with the Syllable DVM. how that is to be read at Length, I cannot say. Nor is there any thing particular in this; since there are several Names in *Gruter*, which occur but once, and are no where else to be found. This Inscription ends in the same Manner, as the former.

I shall now procede to inquire briefly into the Design and Use of these Plates. The antient Pagans had not only their national but domestic Deities, whom they addressed to in private, and sometimes carried their Images about with them, as their Guardians and Protectors (b). And it appears to have been a Custom among them to place their Images in Shrines, made in the Form of Temples, both for public and private Devotion. The *Tabernacle of Moloch*, mentioned in the *Acts of the Apostles* (c), is generally taken to have been of the former Sort. And *Herodotus* informs us, that the *Aegyptians* upon a solemn Day carried in Procession the Image of a Godess,

(a) Lib. IV. Carm. 8. (b) *Ammian. Marcell. Lib. XXII. c. 13.*
 (c) Chap. VII. v. 43.

Godeſs, ſaid to be the Mother of *Mars*, in a wooden Temple gilded over, which was drawn in a Chariot (*a*). Such Shrines are mentioned likewiſe by later Writers. And others of a leſſer Size ſeem to have been made in Imitation of them for private Uſe. The *ſilver Shrines of Diana*, mentioned alſo in the ſacred Hiſtory cited already (*b*), are by moſt Interpreters ſaid to be of this kind. And Mr. *Kemp* had in his Collection of Antiquities one made of Braſs, but five Inches high, with a Godeſs, ſuppoſed to be *Iſis*, ſitting in it; as it is deſcribed in the printed *Catalogue* (*c*). Another of the ſame Deity, but of a different Form, and ſomewhat leſs, is now in the Poſſeſſion of *James Weſt* Eſquire, a worthy Member of this Society. And ſometimes they were placed in the Monuments of deceased Perſons, an Inſtance of which we find mentioned in an Inſcription publiſhed by *Reineſius*, which is there ſaid to have been made of Marble (*d*). The Perſons imployed in making thoſe ſacred Images were called by the *Greeks* ἀγαλματοποιοί, and by the *Romans* *Sigillarii*, as we find in the antient *Gloſſaries* (*e*). And one of theſe Artiſts is mentioned in two Inſcriptions of the *Britannia Romana*, where he is ſtiled SIGILLARIUS COLLEGII LIGNIFERORVM (*e*), who are more uſually called by the *Greek* Name DENDROPHORI; Part of whoſe Buſineſs might be to carry, or attend, the Shrines in their Proceſſions at public Fetiſtivals.

Whether

(a) Lib. II. cap. 63. (b) *Aſſs* XIX. 24. (c) *Monument.*
Kempian. Par. I. pag. 6. (d) *Claff.* XIII. num. 64. (e) In
 voce ἀγαλματοποιοί. (f) Pag. [354.]
 Z z z

Whether or no these Plates ever belonged to Shrines, I cannot venture to assert; but I am rather inclined to think, they did not; except perhaps the first, which from the Largeness of its Size, and having no Image stamped upon it, but only a written Inscription in Honour of *Mars*, might possibly have been laid over Part of a wooden Shrine, within which the brass Image was placed, that was found with it. As for the rest, I imagine they were designed as partial Representations of Shrines for the Use of private Persons; having only the Front of them with the Image of the Deity placed in it; which being fixed upon wooden Tablets might either be set up in their Houses, or carried about with them, in Devotion to those tutelar Deities. And when any fortunate Event happened, which they attributed to the Success of their Addresses made to them, they might sometimes express their Acknowledgement of it by hanging them up in their Temples, among other Donations, making them a sort of votive Tables. That they were intended for some such Uses seems the more probable, from the Number of them found together. They have a Similitude with the Reverse of many *Roman* Coins, where the Images of their Deities are represented in the same Manner; from an Imitation of which they might perhaps be introduced at first, as well for Cheapness, as ready Convenience, in some of the more remote Provinces. And it is very remarkable, that no two Impressions upon these Plates are in all respects exactly alike; as we do not often meet with two *Roman* Coins struck from the same Die.

As to the Time, when the Plates were made, the Inscription upon the first fixes it to the Reign of *Diocletian*; and as not only the Characters of the other Inscription exactly correspond with that, but also the Manner of the Work upon each Plate is the same; it is highly probable, they were all made about the same time, which was near the End of the third Century. And to this likewise the Form of the Letters, particularly A and M, very well agrees. Nor ought it to seem strange, if more of them have not been preserved; since from the Nature of them they appear so liable to be destroyed, either by the Injuries of Time, or for the sake of the Silver.

Gresham College, April 2,

1745.

John Ward.

III. *A Letter from Gowin Knight, M. B. to the President; concerning the Poles of Magnets being variously placed.*

Honoured Sir,

London, April 3. 1745;

Read April 4.
1745.

THE favourable Reception which those magnetical Experiments met with, which you lately did me the Honour to communicate to your Learned Society, (see Tr. N^o 474. p. 161.) encourages me to hope, that the following Facts are remarkable enough to merit their Attention.

I. I cut a Piece of natural Loadstone into the Shape of a Parallelopiped, 1 Inch $\frac{8}{10}$ in Length, in Breadth.

Breadth $\frac{4}{10}$ of an Inch, and $\frac{2}{10}$ in Thickness : Its Weight was 3 Drams and 10 Grains. In this Stone I placed the magnetical Virtue, in such a Manner that the two opposite Ends became, both of them, South Poles; and the Middle was, quite round, a North Pole.

2. Another Stone was in Length 1 Inch $\frac{1}{10}$, in Breadth $\frac{7}{10}$, and in Thickness about $\frac{2}{10}$ at a *Medium*, it being thicker at one End than at the other : Its Weight 1 Dram 57 Grains. The 2 opposite Ends of this Stone I made both North Poles, and the 2 opposite Sides South Poles.

3. An irregular Stone, that weigh'd about 5 Ounces and a half, had 2 broad flat Surfaces opposite to each other, at the Distance of 1 Inch and $\frac{3}{10}$. I made half of each of these Surfaces a North Pole, and the other half a South Pole; so that the North Pole of one Surface was opposite to the South Pole of the other Surface, and *vice versa*.

4. I took a Stone of a pretty good Kind, that had a Grain very apparent, running the lengthways of it : It was 1 Inch $\frac{4}{10}$ in Length, 1 Inch $\frac{3}{10}$ in Breadth, and its Thickness at the Sides was $\frac{6}{10}$ of an Inch; but in the Middle $\frac{7}{10}$; it being tapered away from the Middle to the Sides : Its Weight was 3 Ounces wanting 4 Grains. At one End of it I placed a North Pole surrounded by a South; and at the other End a South surrounded by a North Pole; so that the Edges of each Surface had a Pole of a different Denomination from that which occupied the Middle.

A great many Varieties of this kind might be easily devised; but these Examples seem sufficient to shew how

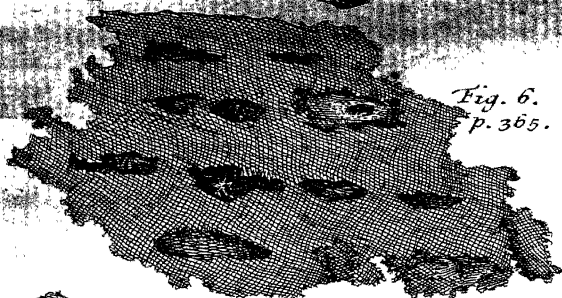
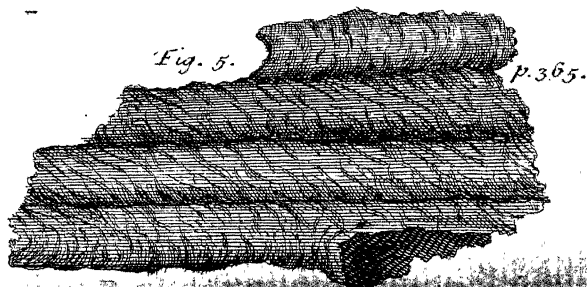
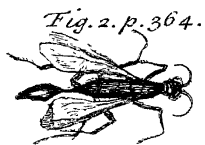


Fig. 3. p. 365.

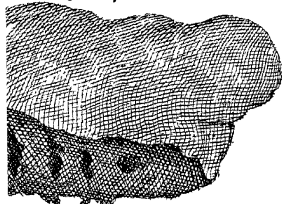
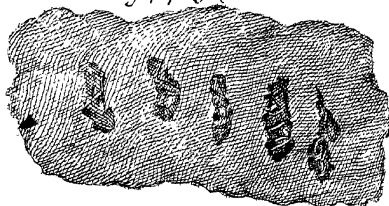
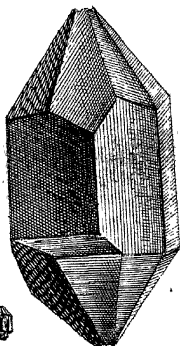


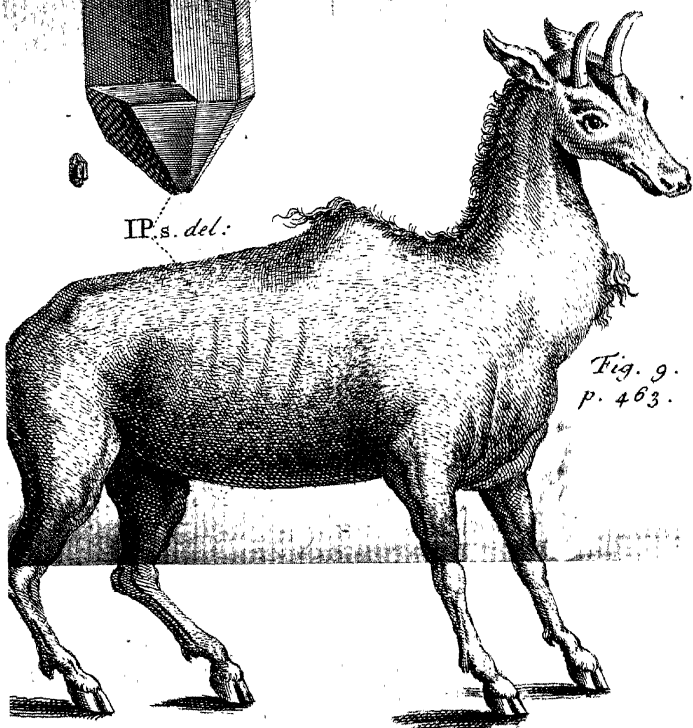
Fig. 4. p. 365.



*Fig. 10.
p. 468.*



IP._{s.} del:



*Fig. 9.
p. 463.*

how manageable the magnetic Virtue is in respect to its Direction; and how defective most of the Hypotheses are, which have been raised to account for the *Phænomena* of the Loadstone.

Your obedient humble Servant,

Gowin Knight.

IV. *An Account of some very curious Wasps Nests made of Clay in Pensilvania; by Mr. John Bartram: Communicated by Mr. Peter Collinson, F. R. S.*

Read April 25.
1745.

MR. *John Bartram*, a diligent Observer of natural Productions, sent me, from *Pensilvania*, two Sorts of curious Wasps Nests made with Clay, which are commonly built against the Timber under the Roofs of Houses and Pales, to shelter them from the Weather. They feed as the Bees, on Flowers; but whether they sting like them I do not yet know.

The plain Clay-Nest is fabricated by a small black Wasp, of the same Species of that in TAB. III. *Fig. 1.* but less, that has a Speck or Stripe of Yellow in its Tail; and the Cells are made four or five together, joining Side by Side to each other. But the Clay-Nests that are so elegantly wrought are built by a purplish black Wasp, such as is figured

TAB.

TAB. III. *Fig. 2.*: After one Cell is formed, they stop it up, and join another to its End, and then add another to that; which makes these wrought Clay Fabrics longer than the plain ones.

Their Method of Working is much alike, and it is very diverting to see them at it: Their Art and Contrivance is wonderful; and, as if it was given to cheer them at their Labours, they make a very particular musical Noise, the Sound of which may be heard at ten Yards Distance.

Their Manner of Working is, to moisten Clay, and temper it up into a little Lump, of the Size of Swan-shot. This they carry to build with; they begin first at the upper End of the Cell, and work downwards, until it is long enough to contain the Nymph or *Chrysalis*: After they have spread out the little Lump in a proper Manner to form their little Fabric, they set up their musical Notes, and return to temper and work up more Clay for the next Course. Thus they continue alternately singing and working, until a Cell is finished; which is made delicately smooth withinside; then, at the further End of each Cell, they lay an Egg; after this, by surprising Instinct, they go and catch Spiders, and cram the Cell full of them: But it is further wonderful to observe, that they only in some manner disable the Spiders, but not kill them; which is to answer two Purposes; first, that they should not crawl away before the Cell is finished; and next, that they may be preserved alive and fresh until the Egg hatches, which is soon.

The Spiders, by wonderful Instinct, are provided for the *Embryo* to feed on: Having stor'd up sufficient for its Support, she very securely closes up the Cell, and then proceeds to build the next in the same Manner.

The Maggot or *Embryo*, having eat up all its Provision, before *Oëtober* prepares for its Change, and spins itself up in a fine soft silken Case, in which it lies all the Winter in the *Chrysalis*-State, until the Spring, when it eats its Way out of its Clay-Dwelling.

April 3. 1745.

P. Collinson.

Since the above Account was read before the *Royal Society*, I have had the Pleasure to peruse Mr. *Reaumur's* excellent Work, Vol. VI. on the Clay-Nests from *St. Domingo*; but as these from *Pennsylvania* differ in many Circumstances, I hope it will not be unacceptable to the Curious to see their Figures, with the best Account I could procure of them. It may deserve our Notice, that these Species of *Ichneumon* Wasps from *America*, like ours in *Europe* of the same Tribe that feed their young with Spiders, very much excel them in the elegant Structure of their Nests.

Fig. 3. A plain Clay-Nest, with single Rows of Cells.

Fig. 4. The Backside of the same, by which it was attached to the Timber-Work of a Building; the
A a a Cells

Cells being partly open, containing some of the *Embryo's* in them.

Fig. 5. Part of a wrought, or wreathed tubulated Clay-Nest, as it appears in Front on the Outside.

Fig. 6. The Backside of the same, where it adhered to the Timber-Building or Pale; some of the Cells being open, disclose the Spiders lodged in them.

Fig. 7. Part of such another Nest as *Fig. 5.*

Fig. 8. The Backside of *Fig. 7.* in which some of the Cells being open discover Spiders lodged in them.

V. *Extract of a Letter from Mr. B--- B---r, containing an Account, in Pounds and Ounces, of the surprising Quantities of Food devoured by a Boy, 12 Years old, in 6 successive Days, who labour'd under a Canine Appetite, at Black Barnsley in Yorkshire. Communicated by Dr. Mortimer, Secret. R. S.*

April 15. 1745.

Read April 25. 1745. **T**HE Boy was regular as other Children, till about a Year ago, when this extraordinary Craving of Appetite first began; which afflicts him to such a Degree, that (they tell us) if he was not fed as he called out for it, he would gnaw the very Flesh off his own Bones; so that, when awake, he is constantly devouring; it can hardly be said eating, because nothing passes his Stomach, all is thrown up again.

[367]

lb	3	Thursday.	lb	3	Friday	lb	3	Saturday
6	4	Water	3	0	Rye	8	0	Milk
2	0	Milk	2	0	Milk	1	4	Bread
3	0	Rye	0	4	Sugar	2	0	Water
0	4	Sugar	2	0	Bread	2	0	Milk
0	8	Treacle	4	0	Milk	4	8	Bread
1	0	Bread	2	0	Water	2	0	Milk
3	0	Milk	2	0	Milk	1	0	Mutton
0	8	Butter	1	8	Meat-Pye	6	8	Water
0	4	Sugar	8	0	Milk	2	0	Milk
1	0	Beef	2	0	Meat-Pye	1	12	Rye
0	4	Bread	4	0	Water	2	0	Beer
6	4	Milk	2	8	Small-Beer	1	4	Pudding
0	8	Bread	0	12	Pudding	0	12	Veal
6	4	Water	0	3	Mutton	0	4	Cheese
3	0	Milk	0	12	Bread	0	8	Treacle
1	0	Apple-Pye	4	0	Milk	0	12	Bread
1	4	Pudding	2	0	Water	0	8	Water
1	0	Veal	2	0	Milk	4	0	Broth
0	8	Meat-Pye	2	0	Water	2	0	Water
6	8	Beer and	1	0	Bread	2	0	Milk
		Water	0	4	Butter	0	8	Butter
7	1	Milk	0	8	Sugar	0	4	Sugar
3	0	Bread	1	4	Meat-Pye	4	0	Beer
3	0	Milk	2	0	Water	1	0	Mutton
3	0	Water	2	0	Milk	1	0	Veal
1	0	Bread	4	0	Milk	0	12	Fruit
2	0	Milk	2	0	Beer			
1	4	Beef	0	10	Bread	5	8	8
4	0	Small-Beer	1	0	Fruit			
1	0	Fruit	2	0	Milk			
<hr/>			<hr/>			<hr/>		
69	8		61	14				
<hr/>			<hr/>					

lb	3	Sunday	lb	3	Monday	lb	3	Tuesday
3	0	Rye	2	12	Bread	5	0	Bread
6	0	Milk	8	0	Milk	8	0	Milk
0	8	Sugar	4	0	Beer	0	8	Butter
6	8	Water	2	0	Milk	2	0	Water
2	4	Bread	2	0	Water	4	0	Milk
2	0	Milk	0	4	Butter	5	0	Hasty-puds.
8	8	Water	0	8	Bread	6	8	Water
4	0	Milk	5	0	Hasty-puds.	0	8	Treacle
2	0	Rye	6	0	Water	1	12	Meat-Pye
2	0	Milk	1	4	Mutton	1	0	Mutton
4	0	Broth	0	8	Bread	1	4	Pudding
1	8	Pudding	1	0	Beef	6	8	Water
2	0	Beer	1	0	Potatoes	2	0	Beer
1	8	Mutton	1	4	Pudding	2	0	Milk
6	8	Water	6	8	Water	2	0	Water
4	0	Milk	4	0	Milk	1	0	Beef
1	12	Bread	0	8	Bread	1	0	Mutton
3	0	Rye	0	8	Treacle	4	0	Beer
2	0	Milk	0	12	Bread	0	8	Sugar
0	8	Butter	2	0	Water	1	0	Fruit
0	8	Sugar	4	0	Milk	<hr/>		
2	0	Milk	3	0	Potatoes	55	08	Tuesday
2	0	Water	4	0	Milk	60	12	Monday
2	0	Milk	<hr/>			77	00	Sunday
2	0	Beer	60	12		58	08	Saturday
0	8	Bread	<hr/>			61	14	Friday
2	0	Milk				69	08	Thursday
0	8	Fruit			Salt	1	0	in the 6
2	0	Mutton				<hr/>		
<hr/>					Total	384	2	Days.
77	0					<hr/>		

VI. *Of an Iliac Passion, occasioned by an Appendix in the Ilion: By the late Claudius Amyand Esq; Serjeant-Surgeon to His Majesty, and F.R.S.*

Read May 2. 1745. UPON the 15th of September 1739.

a Lad about 10 Years of Age, seemingly in perfect Health, after drinking some four Small-Beer, complained of a violent Colic; which increasing with great Tension of the Belly, and continual Vomitings of Excrements, he died three Days after of a *Miserere*, ending in a Mortification of the inflamed Guts.

He was suspected to have been poisoned; which occasioned Mr. *Maccullough's* being sent for to open him. All the large Guts were found empty, up to an *Appendix*, or a hernious Expansion in the *Ilion*, about three Inches long, and of the same Dimensions with the Gut itself; which was so contracted and shut by a Spasm, that as nothing could pass downwards, so all the *Fæces* were retained in the intestinal Pipe betwixt this and the Stomach, which was greatly distended as far as the *Pylorus*; the Opening into the Stomach there, and that in the Guts below, which were contracted and spasmed, being so close, as hardly to admit of a small Probe.

The frequent Vomitings of Excrements, during Life, shew'd, that the Stricture at the *Pylorus* had occasionally given Way to their coming into the Stomach; but, as these were thrown up as soon as let in, this *Viscus* was found as empty, as the Guts were below the strangulated Gut; thro' which

which nothing had passed during the Course of the Disease.

This *Appendix* of the *Ilion*, more capacious and longer than that usually observed in the *Cacum*, being supported by no Mesentery, lay loose and floating. At its Opening into the Gut, it made an acute Angle, determining the Course of the *Fæces* from the upper Gut into it, and obstructing their Descent into the natural Pipe; where the Current was made difficult, from a Defect in some Segment of the Fibres inservient to the vermicular and peristaltic Action; which, likely, was the Occasion of the Spasm, the contracted Fibres of the Gut having no Antagonist above, and the compelling Force to conquer the Resistance being wanting.

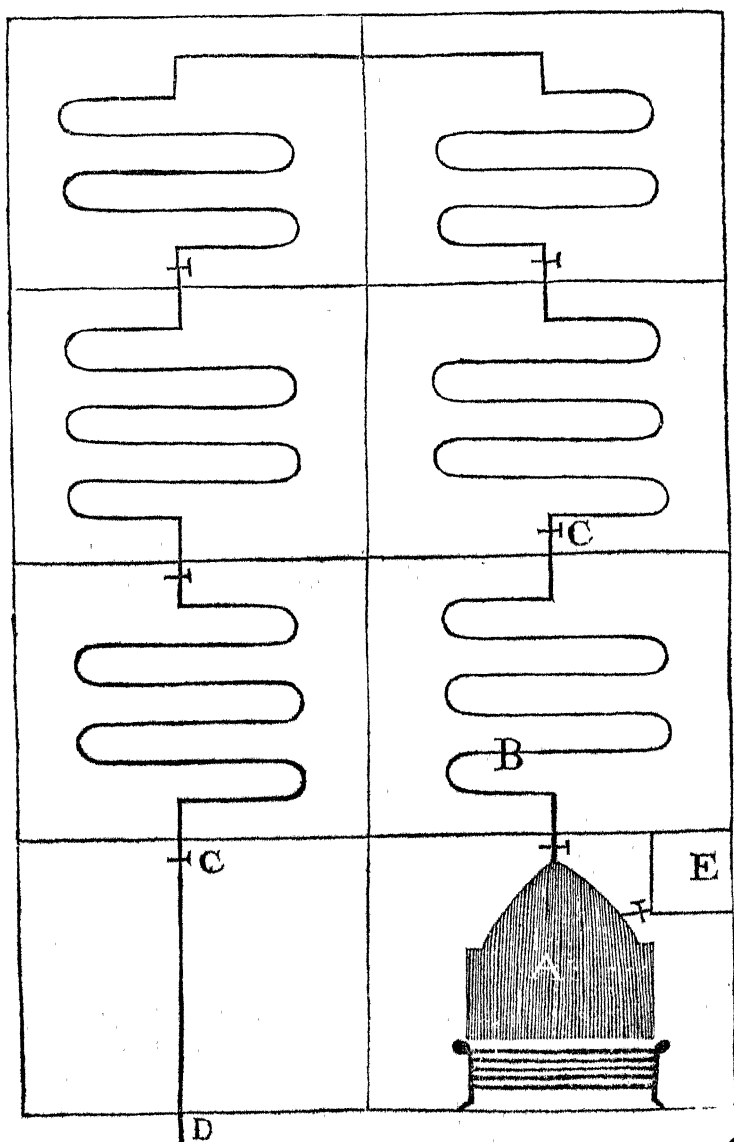
In the Liver there was a steatomatous Tumour stretched out thro' its Substance, containing in its *Cystis* about eight Ounces of Matter; but so disposed, that the Course of the Fluids and Juices in and out was not impeded.

VII. *A Proposal for Warming Rooms by the Steam of boiling Water conveyed in Pipes along the Walls: And a Method of preventing Ships from Leaking, whose Bottoms are eaten by the Worms: By Colonel Wm. Cook.*

Read May 2.
1745.

AN Engine for giving a sufficient Heat to all the Rooms in a House from the Kitchen Fire.

Fig.



A, Is a Copper with a Still-Head.

B, Is a Lead or Copper Pipe fixed to the Head of the Copper, thro' which the Steam from the boiling Water heats: In its Passing thro' the eight Rooms the Pipe is fixed to the Wall or Side of the Room in the Place of the Chimney.

C, C, Are Stop-Cocks, by which the Steam may be suffered to pass fast or slow, as you please.

D, Is the Vent for the Steam to pass out at.

E, Is a Cistern of Water to replenish the Copper as it boils away.

WHEN your Ship's Bottom is so eaten by the Worms, that she is no longer fit for Service, try the following Method; *viz.* First calk well the inside Planks or Lining; then fill the vacant Spaces between the Timbers, and the out and inside Planks, with boiling Pitch or Resin, so high as the main Gun Deck.

The Pitch, being put in very hot, will run into every the smallest Cranny, and make the Ship as tight as a Bottle. By this the Ship is ballasted; there will be no Room left for Vermin, as Rats, &c. and the Pitch will serve for other Uses when taken out; therefore it will be but little Expence.

VIII. *De Lapide Osteocolla Inquisitio, Auctore*
 Ambrosio Beurero *Neurembergensi*; *com-*
municata per Dominum Petrum Collinso-
num, R. S. S.

Read May 9. ^{1745.} **L**APIS Osteocolla variis nominibus

insignitur; communissime tamen *Ostiocolla* sive *Osteocolla* dicitur, quod vocabulum e duobus græcis ὀστέον, *os*, & κόλλα, *gluten*, componitur: alias etiam audit lapis *ostites*, *ollosteos*, *ossina*, *ossifana*, *ossifraga*, lapis *Asiaticus*, *Pierre de Monti*, lapis *Morochius*, flores arenæ, fossile arborefcens, lapis *fabilis*, lapis *arenosus*; *Germanico* idiomate, *Steinbruch*, *Bruch-Stein*, *Steinbruch-Stein*, *Knochen-Stein*, *Hand-Stein*, *Bein-heyl*, *Beinbruch-Stein*, *Bein-Quell-Stein*, *Bein-Knochen-Stein*, *Stein-Blumme*.

Majores nostri nullum habuere hujus lapidis notitiam, putantes esse ossa petrefacta; aliis speciem gypsi credentibus.

Osteocolla autem provenit in ducatu *Crossensi*, *Silesiæ*, *Pomeraniæ*, *Hassiæ*, *Saxonie*, *Poloniæ*, *Darmstadii*, *Heidelbergæ*, *Spiræ*, *Jenæ*, *Megalopoli*, in marchionatu *Brandenburgico* ad *Bescoviam*, *Snonebergam*, & *Drossenam*.

Tellus, ubi provenit, semper est fabulosa & sterilis; passimque nihil aliarum arborum aut plantarum conspicitur, nisi meræ populi: nullæ autem, quod quidem tradunt, inveniuntur in vallibus, neque oriuntur

a scaturigine, neque unquam in viis lutosiis deprehenduntur.

Kräutermannus nonnemini quicquam obtulit domus atque arcis figuram sistens: magis autem tophus, quam Osteocolla fuisse videtur. *Mercatus* quoque vera ejusdem caruit notitione, dum petrefacta & tophos calcarios ejusmodi nomine insignivit, quorum hi *Hermanno* iudice magis bolaria aut cisti sunt.

Quod attinet ad ejus originem, provenit in modo memorata tellure fabulosa ad quorundum pedum profunditatem, & radicis gerit figuram. Maximos vix duabus manibus complectaris, reliquæ sensim sensimque sunt minores. Quantum ad consistentiam, Osteocolla sub terra adhuc latens nunquam est dura, sed semper mollis & limosa, adeo, ut si digitis teratur, prorsus sebacea & pinguedinosa sit, tum autem siccata albescat, ut calcarium quicquam. Ut sub terra reperitur, partim cana est, partim flava aut alba, exteriusque sabulum eidem copiosum adhæret. Propter mollem consistentiam non omnino integra eximi potest, ut veram radicis figuram gerat, nisi artificiosissime aggrediaris negotium, & nonnullas forte septimanas, aut aliquot menses, in illud insumas, siquidem raptim ablato sabulo rumpitur: unde sequentia sunt observanda, ut,

1. Laboriose & patienter quærat:ur :
2. Sabulum lente abstergatur :
3. Procul a radice auferatur :
4. Dispicatur, annon parvæ radices secundariæ irregulariter propullantes adsint, quas cavendum est, ne decutias :

5. Non multæ simul fodiantur, sed potius aliquantum moræ illis indulgeatur, quæ siccentur atque durecant: tum,

6. Fovea asseribus obtegatur, ne quid ei fortuito illabatur aut pluvia illam conspergat:

7. Fossio non est repetenda, nisi exstantibus probe duratis:

8. Calida sicca atque leni tempestate asseres rursus auferantur, ut eo citius exsiccentur:

9. Fossio é longinquo incipiatur, cum inferius plerumque aqua inveniat.

Osteocollam intus cavam esse, plurimi quidem norunt auctores, in eo tamen invicem differunt, utrum illa in vegetabilium an mineralium numerum sit referenda. Plurimi veterum eam ad ossa retulerunt metamorphosin experta, alii contra negant, quum perfecta ossium fragmenta non inveniantur, neque in chemia ullum partium animalium vestigium compareat. *Erasmus* admodum verisimiliter de ea scripsit: qui Osteocollam pro ossibus non agnoscunt, minerale eandem agnoverunt é fabulo genitum, & jure quidem suo; *Dn. Prof. Teichmeyerus* eam margam nuncupat; *Dn. rei metallicæ consiliarius Henckelius* ad mineralia eam refert, generationem autem ejusdem reticet; *Dn. Prof. Junckerus* eam in fabulo gigni perhibet, non autem addit, num ex arborum stemmatis, an e radicibus proveniat. Ego eam radicem puto, cui arena adhærescit, quæque sensim ita generatur. Et quamvis satis Osteocollæ inveniat, nihil tamen unquam ligni arborei aut saltem viride conspicitur supra terram extans; & licet de origine hujus Osteocollæ ejusque arboris, cujus radix in Osteo-

collam jam degeneraverat, exacte inquisiturus essem, diu tamen id mihi non contingerat, nisi ad ultimum; ubi tandem tamen ad *Zernium*, haud procul a *Zossena*, aridum ejusmodi ramulum & viridem frondem conspexi, quod arbori, superius adhuc lignosæ, inferius autem in meram Osteocollam jam transmutatæ adhæferat, quæ accuratius paullo examinata species Populi erat.

Origo ejus igitur quærenda est in populo nigra, cujus arbore aut stemmate decisis aut petrifactis & corruptis, Osteocolla radici sensim accrescit, primario aut furculis.

In omnibus vero etiam Osteocollæ partibus inveniis, iis adhuc aliquid lignosi inesse, utut jam putrefacti, eoque elapso eas perforari, ita uti ossi similes appareant. Nunquam autem in arboribus prope illas atque in eadem tellure collocatis Osteocollam inveni: quodsi tellus esset in causa, in pinis, betulis, & similibus reperiri oporteret. Hoc vero docet,

1. Nunquam fere Osteocollam in regionibus *Salinis* deprehendi:

2. Credendum esse, ubi Osteocolla invenitur, ibi semper antea populos extitisse:

3. Omnes Osteocollæ fossiores visuros, eam fuisse radicem:

4. Unum adhuc criterium suppeditari posse, ubi quid Osteocollæ invenitur, ibi ordinariæ aliquid calcariorum ossium prominere videri, quæ florem exprimere videantur: unde multis venit in mentem eam crescere & florere.

Osteocolla igitur est sub sabulo, semper autem eo tantum loco, ubi radices sitæ erant, quibus adhærescens

rescens sensim obduruit, atque in quantum eminuit, in alborem se induit; quodsi quid ejus dissimile invenitur, casu fortuito illi fortasse afflatum est. Radice inventa, spithamam unam profundius fodi potest, & certo Osteocolla reperietur. Osteocolla igitur, quam diu sub terra manet, mollis, aut circiter instar calcis maceratae fabulo permixta: ubi vero humor in aërem abiit, sensim etiam magis indurescit.

Ad generationem ergo requiritur

(a) Radix populi:

(b) Si ne radix populi quidem conspici queat, ope tamen distillationis vegetabile in oleo empyreumatico demonstrari potest:

(c) Ad generationem multum confert acidum salis fabulo arte unitum:

(d) Ut & arena subtilis, quum in chemia notum sit, in arena semper aliquid acidi remanere, atque adeo semper lapidescens quid adesse, ut ita, vi dictorum, acidum salis una cum multo humore fabuli materiam constituat, nisi quod hic liber aëris accessus adhuc desit, quum alias in terra jam in lapidem transformatum fuisset.

Hoc inde apparet, quia,

1. Massa, simulac aëri committitur & ficcatur, indurescit:

2. Destillatio aliquid empyreumatici petrolei & partibus vitriolicis & bituminosis constituti ostendit:

3. Si oleum vitrioli Osteocollæ affunditur, acidum salis communis inde secedit:

4. Osteocollam vero non esse calcem, inde probo, quia ego nullo modo hanc elicere potui.

Ego Osteocollam variis menstruis aggressus sum visurus, quantum cujusque pondus in unoquoque eorum solveretur : unde semper Osteocollæ drachmam dimidiam atque cujusque menstrui unciam dimidiam hoc fine sumsi, sequentia ibi observavi :

- (a) Oleum vitrioli iv. ejus grana solvissē, quæ solutio erat coloris flavi, residuum vero ex albo flavescētis :
- (b) Spiritus vitrioli omnia in formam salinam redegit :
- (c) Spiritus nitri ejus scrupulum i. grana iv. &
- (d) Acidum salis communis scrupulum i. grana vi. solvissē :
- (e) Aqua regis vero scrupulum i. grana iv. utramque autem solutionem flavam evassē, residuum autem ex albo flavescēns :
- (f) Acetum destillatum etiam scrupulum i. cum dimidio solvissē, solutionemque ejus subflavam evassē ; residuum vero, ut cætera, a menstruo quoad coloris mutationem immunia mansissē.

Osteocolle itaque, ut in officinis usurpatur, minera est putanda, imprimis vegetabili inde separato ; potest dici terra calcaria ; non autem mutat syrupum violarum.

Ope destillationis igne aperto factæ suppeditat spiritum urinosum ; alcali fixo affuso effervesceat, simulque prodit spiritum urinosum ; residuum aquæ admiculo in lixivium redactum nihil produxit salini ; simul etiam unam residui partem in calcem redigere studui, nullam autem calcem obtinui. Quodsi vero oleum vitrioli Osteocollæ affunditur, acidum salis communis

communis inde secedit. Osteocolla alcali calcinata opacum vitrum constituere videtur, rursus autem in aquam resolvi potest, indeque verum vitrum nondum est putandum; Osteocolla retortæ tubulatæ indita, lenique igne imposita, oleoque vitrioli per tubulum affuso spiritum salis inde liberat, qui etiam destillando obtineri potest. Ego etiam obtentum hunc spiritum salis, hanc ob rationem sale alcali fixo saturavi, & sal commune regeneratum effeci, postea siccatum retortæ rursus immisi, oleum vitrioli affudi, iterumque in retortam indito, oleum vitrioli infudi, denuoque destillando spiritum acidum salis communis obtinui.

Summa Osteocollæ basis est sabulum; nonnulli volunt lac lunæ, medullam *Saxorum*, & lapidem Osteocollæ unum idemque esse, quod vero falsum est. Si quid rubicundi in Osteocolla fuerit repertum, martiale quid in vicinia adfuisse. Usus ejus medicus est absorbens; unde a nonnullis ad fluorem album sedandum adhibetur.

IX. *A Letter from J. Cookson, M. D. to Mr. Latouche at Little Chelsea, concerning the Boy who has an extraordinary Boulimia, or craving Appetite : Communicated to the Royal Society by John Martyn, F. R. S. & Prof. Botan. Cantab.*

S I R, *Wakefield, April 24. 1745.*

Read May 9. 1745. I WAS desired, by your Friend Mr. Arnet, to transmit to you what I could collect relating to the Boy at *Barnsley* [6 Miles from *Wakefield*]; so please to accept of the following, with the inclosed Account of his Eating and Drinking, taken by a Friend of mine, for six Days successively.

Matthew Daking, a healthful and sprightly Boy, about ten Years old, was, about fifteen Months ago, seized with a Fever, which continued above a Fortnight. In the Beginning he had frequent Provocations to vomit, which induced his Apothecary to give a gentle Vomit of *Ipecacuanha*. The Reachings continuing, he gave him another: They seemed to operate well, but yet did not answer the End in settling his Stomach: However, the Fever gradually went off, but the Vomiting rather increased, notwithstanding some other Methods were used.

He then began to have a craving Appetite; to satisfy which he was indulged in Eating and Drinking more plentifully, but always vomited most of what he had taken, almost immediately. His Appetite kept

kept increasfing, fo that, in a few Weeks his Eating was come to the Pitch you now fee it in.

Thus he has continued above a Year. His Urine and Stools do not exceed thofe in Health; fo that he vomits moft of what he takes in.

He has tried crude *Mercury*, and all Sorts of Medicines, and Mineral Waters. At prefent he looks pretty well in the Face, and is chearful; but has loft the Ufe of his Legs and Thighs, which are much emaciated. He is fometimes fo hungry, that he fays he could eat them all: He often wifhes he were in the King's Kitchen.

One Pig was fed with what he had vomited, and was fold in the Market: But the Country-People, getting hold of the Story, put a Stop to the Feeding of any more.

To account for the Diforder I am much at a Loſs, fo ſhall not trouble you with my Conjectures. I am

Your moſt obedient Servant,

J. Cookfon.

By the Journal annexed it appears that he eat the following Quantities of Various Sorts of Food both Meat and Drink, as is ſpecified in the other Journal, which I have given at large, p. 367, and 368.

	lb	ſ		lb	ſ
1745. Apr. 4. the whole } Quantity amounted to	65	8	Apr. 7. it amounted to	76	12
Apr. 5.	60	14	Apr. 8.	to	60 8
Apr. 6.	58	8	Apr. 9.	to	55 8
			Total in fix Days	371	16
			Salt	1	10
				379	16

N. B. He died a few Months after, quite emaciated.

G. M.

X. Philippi Frederici Gmelin. *Med. Licent.*
 Wurterbergenfis ad Martinum Folkes *Reg.*
Soc. Præsid. Epistola de Radice Ipecacu-
 anhæ *Observationes quasdam medico-physi-*
cas, & de fallaci Visione per Microscopia
composita notata nonnulla continens.

Tubingæ, Cal. Maii, 1745.

Observationes medico-physicæ de radice
Ipecacuanhæ.

Presented May
9. 1745.

ALTER jam incipit agi annus, a quo
 ex itinere literario per *Germaniam,*
Hollandiam, & Angliam domum redux, praxi ope-
 ram dedi medicæ; ubi quidem, dum neo-practicus
 non potest non passim alieno arare vitulo, miratus
 sæpe sum, quod tot occurrant formulæ atque medica-
 mentorum compositiones, quas simplicia ingrediuntur,
 de quibus a priori ne minimum quidem demonstrari
 potest, quod huic illive possint satisfacere indicationi;
 a posteriori autem sæpe ad summum hoc elucescit,
 quod mixta efficacioribus medicamentis impedire
 nihil, vel nocere possint; caute feci ego hanc in rem
 experimenta plurima, & in omnibus casibus mihi ob-
 viis hætenus eo allaboravi, ut exquirerem efficacis-
 sima, quibus vel a priori hanc illamve adscriptam in-
 esse virtutem probabile judicabam, vel, quibus eam
 inhærere indubitatis sciebam observationibus, ita
 inveni tantum non semper, quod idem, imo plus
 ordinario effecerim paucis medicamentis, quam qui-
 dem alii farragine ipsis etiam ægris odiosa præstare
 solent; idque verum deprehendi etiam in morbis per-
 tinacissimis,

tinacissimis, & acerbissimis; at neque hic solum displicuit mihi mos multorum practicorum, sed & alius inprimis, quando scilicet simplex aliquod passim magna præscribunt dosi, ægro & molesta, & pretiosiore, cum tamen effectum certo certius majorem minoribus sumtibus, & ex multo minore medicamento quantitate potuissent obtinere. Illustrabo jam hoc exemplo radicis Ipecacuanhæ, quæ a multis annis famosa est ob virtutem singularem emeticam.

Docuit experientia certa, mineralia emetica tuta nunquam esse, licet efficacia satis forent, quemadmodum apud nos duo vel tria saltem tartari emetici grana vel robustissimo homini vomitus atrocissimos & creberrimos excitant sæpius, cujus rei ratio forsan non inepta est specificum corporum omnium metallicorum, minima etiam ingestorum quantitate pondus, quod æquilibrium soliditatis nunquam servare potest ad tenuissimos villosæ ventriculi & intestinorum tunicæ penicillos, quos nimium gravat, premit, acturos impedit, nec facile ad activitatem decentem redire finit; hinc igitur de vegetabilibus cogitare vomitoriis, quæ, ut omnia vegetabilia, sperarunt facilius ab animali oeconomia superatum iri, quemadmodum etiam revera res sese habet, quæque multo sunt tutiora.

Deprehenderunt autem inter ea commodissimam & securissimam Ipecacuanhæ radicem, cujus descriptionem utpote tot in libris obviam hic non repetam, unicum hoc adjecturus, quod apud nos inprimis officinalis sit illa flava, quam *Boeclerus* in novissima editione cynosuræ materiæ medicæ allegat: est autem flava, non externa superficie, quæ terrestris magis esse dici posset coloris, sed interna corticis facie, quæ præcise colore corticis ligni sassafras, ut is intus est, ex-

primit, nervus autem totus non tam pallidus, quam vere candidus est, in medio sectus medullam offerens mollissimam. Radicem hanc diversa dant dosi pulverisatam, alii ad ℥j. alii ad ℥ij. & ℥j. ascendunt; sunt etiam, qui jubent ejus ℥j. ℥ij. cum aqua distillata infundi, & sic pro una dosi hauriri: scribit etiam *Boeclerus*, quod ejus potissima virtus in cortice resideat; alibique monet, dubium esse nullum, quin refinosa quædam tinctura & alia possint ex eo extrahi, de ipso interim modo non sollicitus. Jam ergo demonstrabo experimentis practico-medicis, quod multo minore dosi, quam ipsa radix, ejus cortex multo validiorem, tutumque tamen præstet effectum, & in pulvere datus & infuso, quin & observationibus insuper microscopiis confirmabo, quod & a priori hoc satis manifestum sit & evidens.

In forma quidem pulveris nunquam simplicem adhibui corticem, sed remixtum cum appropriatis incidentibus, absorbentibus, leniter adstringentibus, tonicisque: formula hæc erat: *℞ Cort. Rad. Ipecac. Elcolacch. citr. per affus. par. aa. gr. vj. Ray. Cort. aut. Scall. sal. Sedac. aa. gr. ij. Magnes. albiſſ. ℥j. m. f. pulv.* pro una dosi, quam æger sumat mane. Assumfit pulvisculum hunc sine omni fere nausea; & exceptum frequenti potatione infusi theæ valde diluti, calidique, vel jusculi carniū calidi: expertus est provocasse 6, 7, 8, vomitus plenos eo mane, pro diversitate subiecti, sine singularibus tamen molestiis, stric-
turis & similibus.

Dedi autem eundem corticem in forma etiam infusi: simplex erat formula hæc: *℞ Cort. Rad. Ipecac. follicite separ. & grosse contus. gr. vj.* His infundantur vini *Nicar. vel Rhenan.* opt. vetust. *℥j. ℞.* Stant
per

per noctem, mane autem pro uno hauriantur vice decantata; effectum observavi eundem; quando æger easdem, ut supra, secutus erat leges, licet interim non promiscue suaserim corticem hunc vel in pulveris, vel infusi sumere forma, cum certe in casu dysenteriae neque fluida in genere conveniant adeo, neque multo magis vinosa, utpote nimis calefacientia; e quibus igitur omnibus prono fuit alveo hæc observatio: cortex radice Ipecacuanhæ multo efficacius agit ipsa radice tota; 2. multo minore id efficit dosi; 3. minore sumtu, & minore nausea, potest assumi radice ipsa tota.

Sed jam omnia hæc, ut promisi, microscopicis etiam sunt a priori probanda observationibus; dicam candidè, quales instituerim: Habui autem *Anglica* & alia microscopia varia, satis bona, & simplicia & composita: compositum erat illud portatile reflectens sine micrometro ex inventione *Benjamini Martini*, quod ex *Anglia* ipse mecum duxi: hujus igitur luminoso foco primum exposui derasi prudenter corticis hujus radiceis exiguum valde frustulum, purum, tenue: apparuit externa superficie fere opacum, admodum inæqualem, coloris ejusdem, cujus inermi videtur esse oculo, nimirum instar sicæ humi; superficies autem interna eadem spectata ratione congeriem offerebat informem massularum brevissimarum, tenuissimarum, ubique interruptarum, acutarum fere omnium, & angulosarum in minimis visibilibus atomis a potiori purpurarum, fere pellucens, instar resinæ alicujus, connexarum tamen interspersis figuræ similis filis fere variegatis, potissimum albicantibus; ad marginem autem veri passim aculei comparebant, omnisque contextus videbatur ipsi similis, quem ex animali œconomia

œconomia describunt in pinguedine anatomici, ubi dicunt, effusum illud oleum esse in cellulas, quæ lanuginosa molli substantia illa cellulosa cohæreant, ita scilicet & hic substantia illa purpurea, resiniformis interfusa videbatur filis albicantibus; atque sic, licet multoties repetitis observationibus videbatur mihi semper comparatus hic cortex vel simplici, vel composito spectatus microscopio, ea saltem cum differentia, quod reflectens omnia distinctius repræsentaret, quam quidem reliqua.

Sed nondum hic subtili subjeci etiam memoratum radicis nervum, qualem habebam sponte fragili, fissili-
que cortice nudatum, quique maximum radicis volumen efficit, licet cortici, utpote siccissimus pondere, cedere multum videatur composito microscopio, is autem inermi jam oculo videbatur passim aspersus esse maculis obscuris purpureis: cupidus fui scire, quid hæ sub microscopii foco essent exhibituræ; intuirus ergo sum frustulum illud, quod tales obtulit maculas: nervus ipse visus est fere convexus æqualiter, cylindricus & fibris longitudinalibus non interruptis, fibimet invicem appollis arte, tenuissimis, candidissimis confans: ubi autem maculæ illæ inermi prius oculo spectabantur, observabat armatus veras massulas pelluentes, ex atra rubras, cylindri convexo insidentes & supereminentes: at hæsitavi num crederem massulas has non supereminere nervo, num vero statuerem inhærere nervo ipsi, & ad ejus pertinere substantiam: dubium mihi movit nuper amicorum aliquis desideratissimus, microscopici observationibus plurimum navans operæ; is, scilicet (liceat mihi ab Ipecacuanhæ radice paulisper evagari ad alia, universalia nimirum de microscopici observationibus notamina!) is, inquam, per jocum, me interrogavit,

terrogavit, sciremne quomodo sigilla ordinaria possent explorari exactissime, an essent accuratæ satis incisæ æri, metallo cuicunque, vel e lapidi? Respondi, Expectaturum me, quid novi mihi esset apportaturus. Regeßit ille, Si foco microscopii compositi, vel & tubi optici, qui 2 vel 3 sive utrinque convexa, sive convexo-plana habeat vitra, expones illud, deprehendes id, quod incisum est & profundum, depressumque, valde convexum, & elatum; quæ autem sunt elata & convexa, depressa: his autem adjecit, quod subinde quoque, sed rarius contingat, ut in eo statu sigilla hæc videat, in quo inermi videntur esse oculo!

Imitatus fui phænomenum; & cum semel focum observâßem, constans mihi videbatur observatio, quotiescunque repetebam experimentum; miratus autem valde sum, dignumque judicavi, quod ulterius prosequerer; feci hoc, dicam autem candide, quid tum observaverim: Aspexi mane non admodum sereno, in conclavi cæterum satis illuminato, die xvi. *April.* hujus anni per partem tubi optici ductilis ultimam horologium portatile, pendens e pariete plano; apparuit totum quantum concavum, intraque ipsum firmatum parietem; aspexi ulterius muscas vivas per parietem decurrentes, apparuerunt eodem modo: intuitus porro sum globum thermometri minorem rubro plenum spiritu, & visus etiam hic est concavus, & intra tabellam firmatus. spectavi eadem ratione nodos vestis coloratos utcunque aliosque aurichalceos arcule aliquique eminentes plurimum, apparuerunt utrique concavi, profundè immerßi vesti & ligno; subjeci ejusdem tubi foco formam minorem cervini capitis e ligno sculptam horizontaliter ad parietem pendentem; fuit & hæc prorsus concava, & parieti quasi ex instituto infixæ.

Tandem

Tandem & contemplatus fui thermometri *Fahren-heitiani* hydrargyro plenum bulbum: is autem naturalē convexitatem non mutavit, uti nec sphaera vitrea vacua inversi thermometri, pendentis e pariete, licet inferior bulbus & hujus spiritu plenus rubro, & adstantis *Fahrenheitiani* spiritu repleti convexitatem amitterent, ut alias reliqua priora omnia; unde fere colligebam statim, albida, vel splendida corpora, non colorata sub hujus tubi foco in naturali apparere statu, in quo inermi sese repræsentant oculo; interim tamen candide fateor, accidisse nonnunquam, ut adstans mihi amicus in aliquibus objectorum contrarias sub iisdem mecum circumstantiis faceret observationes, imo, ut ipse altero die obscuriore contrarias prioribus instituerim & hesternis, igitur supposui oppido subfuturas forte, (quamvis observatio cum sigillo esset constantissima) aliquas circumstantias, sub quibus objecta hæc apparerent tam perversa, forte non observatas: itaque operam dedi, ut certas vel tandem definire possem leges, sub quibus perversa hæc objecta semper apparerent his exposita focis, & alias sub quibus constanter eo sentiet exhiberent modo, quo inermis ea distinguit; cuius feci multa experimenta in hanc rem & multoties, denique voti factus ex parte compos.

Quoties videlicet objectum aliquod, super plano eminens, utcunque coloratum, nec album, nec splendens, ita sum intuitus, ut oculum, tubumque opticum directe prorsus illi opponerem, a potiori elata mihi videbantur depressa, hæc elata, ita hoc mihi accidebat in sigillo, quoties per tubum perpendiculari situ manu sustentatum id ipsi proxime obvertebam e directo, ut omnis ejus superficies fere regeret vitæum
tubi

tubi ultimum orbem, ita & sub microscopio mihi occurrebat composito; reliqua vero huic applicare vel supponere non potui; quoties dein aliquod reliquorum objectorum e plano perpendiculari dependens perpendiculariter intuitus sum ita, ut tubus horizontali sustentatus situ e directo ipsi obverteretur, idem mihi accidit semper, neque mutata est visio, ubi objectum oblique propendebat etiam vel horizontaliter, fuitque jucundum spectaculum hoc, cum fumifugium considerarem tabacarium, e pariete oblique pendens, bulbo murrhino nivei candoris, tubo corneo fere nigro constans, bulbus naturalem servavit convexitatem, tubus contra profunde depressus, & parieti tantum non immersus apparuit; quin & observavi hoc, ubi v. c. horologium portatile horizontali plano horizontaliter imponebam, tumque perpendiculari visione intuebar, proxime ad fenestram, non amplius adeo depressum visum fuit, & circumfusus annulo quodam umbrato; unde suspicio mihi nata fuit, omnes has fallacias per umbram oriri, quemadmodum pictores possunt imaginem aliquam elatam repræsentare, si super fundo sit picta multum ipsa lucidiore, profundam, si super obscuriore fundo, id est artificiali circumdatam, umbra: Dein & hoc monendum est, ubi elatum objectum erat, v. c. inter fenestras positum sic, ut a latere potuerit undique illuminari, visum priori modo, non mutavit convexitatem: Detexi autem tandem omnes hæc fallacias hoc modo, vel potius inveni methodum, qua objecta naturali sua convexitate apparerent semper; fuit autem hæc: Si quod objectum e pariete pendebat, vel illi quocunque situ proxime contiguum erat, illud aspexi prorsus a latere ita, ut tubum non opponerem e directo, sed infra ipsam eminentiam proxime

ad planum aliqua distantia; ita nodos arculæ contuebar, ita reliqua, & semper mihi apparuerunt vera naturali convexitate: in exploratione sigilli procedebam sic; Tenui sigillum ita, ut omnis ejus orbis prorsus esset perpendicularis, vel & magis paulo inclinatus, tum tubi oram infimam applicui exacte margini superiori disci sigilli, ut faceret obtusum cum sigillo angulum tubus, hoc facto servato sollicitè situ eodem lentissime ab ora sigilli movi tubum super sigilli facie, ita conspexi sigillum vera naturali facie semper; cur autem præcise hæc omnia sic accidant, ego non determino, uti & rationem, cur alba, vel pellucencia splendida corpora non colorata, utcunque super aliquo eminentia plano ab hac visionis lege offerant exceptionem, nec appareant depressa modo jam memorato spectata, uti alia? Sagacioribus relinquo decidendum.

Jam vero facta hac digressionem, quam si nimis prolixa est, deprecor, redeo ad Ipecacuanhæ radices corticem: Cum igitur scire cuperem, num illæ massulæ pertinerent ad corticem, num ad ipsum nervum, subjeci illas ~~ad~~ ^{foco microscopiorum ali-} quorum simplicium, & idem prorsus observari, supereminere scilicet eas nervo, & ad corticem potius esse referendas, quam nervum.

E quibus igitur singulis etiam microscopis hujus corticis aspectibus concludo patere a priori etiam suo sensu, corticem non posse non multo efficaciorum esse ipsa tota radice, si huc trahamus inprimis indubitatam illam, & toties confirmatam practicam experientiam, qua constat purgans vegetabilium principium residere præcipue in resinosa illorum parte: secundo autem, si deinceps cogito illam universi corticis sub microscopio faciem, quæ contextum exhibet atque con-

geriem

geriem e plurimis quasi spiculis brevissimis, acutissimis, minimis: habebitur etiam explicatio illius hypothesis, qua vim purgantium & emeticorum præcipue ratione mechanica explicando, fingendo spicula talia, cunctorum rationem habentia (forsan & aculeorum, quales in viva inermi etiam oculo apparent urtica, fatis sensibilibiter vivum, cui applicantur, humanum corpus irritantes), quæ motu ventriculi peristaltico iteratis vicibus musculosæ illius tunicæ, utut mediate applicita & impacta, tumque perpetuo hoc motu huc agitata illucque, motrices fibras pungant, stimulent, & ad insolitam fortio rem contractionem excitent, licet interim ego certe nolim adhuc hypothesein hanc pro veritate vendere demonstrata, utut appareat speciosa etiam his microscopicis observationibus!

XI. Remarks on the Operation of Cutting for the Stone; by Claud. Nic. Le Cat, M. D. F. R. S. Surgeon to the Hotel Dieu at Rouën, and Royal Demonstrator in Anatomy and Surgery. Translated from the French by T. S. M. D. F. R. S.

ARTICLE I.

*Read at several Meetings;
finished May 16. 1745.*

ALL the Methods of Cutting for the Stone may be commodiously divided into the *high Apparatus* [or Operation] wherein the Incision is made above the *Os Pubis*; and into the *low Apparatus*, wherein the Incision is below the *Os Pubis* and *Scrotum*. In the first, the Stone is extracted through the upper Open-

ing of the *Pelvis*: In the second, the Stone is taken out thro' the lower Opening of the same *Pelvis*.

The *low Apparatus* [or Way] may be likewise divided into *direct* and *lateral*. The *direct* is the *greater Apparatus* [or Cutting on the Staff]: The *lateral* is of four Sorts.

The *lateral Apparatus* of the first Sort is that which is done without the Staff [or *Catheter*], and wherein the Operator has no other Guide but the Stone itself, which is pushed forward, as much as possible, towards the *Perinaeum*. This is called the *lesser Apparatus* [or Cutting on the Gripe], which *Celsus* has described. It is the oldest of all the Ways of Cutting, and may be look'd on as the Source of all the other Sorts of the lateral Operation.

The second Sort of lateral *Apparatus* is that wherein the Operator makes use of a grooved Staff, on which he cuts the [inner] End of the *Urethra* lengthways, and makes laterally on the Inside of the Prostate, and on the Neck of the Bladder, an Incision about two Lines deep, or a Sort of laying open, which only makes way for the Dilatation or Laceration. This is the Method of Cutting which I use, after having had it from Mr. *Morand*, who learned it of Mr. *Chefelden*. And it is to this I have endeavoured to give the Improvements which are already known in the World, and which I intend speedily to publish. In my Opinion, one may rank in the same Class the Method whereby Monsieur *de la Peyronie* lays open laterally the Neck of the Bladder, with Instruments differing but little from those of the greater *Apparatus*. [For] this Method, as it has been communicated to me by that great Surgeon, scarcely differs from ours but in the Instru-
The

The third Sort of lateral Operation is that, wherein, under the Direction of the same grooved Staff, he cuts quite thro' the Neck of the Bladder, the Prostate, and the [inner] End of the *Urethra*. This is, properly speaking, Frier *Jaques's* Method rectified; that which Mr. *Cheselden* practised last, that of Mr. *Sharp* his Disciple, and, probably, that of Mr. *Rau*; if, after all, it is not of the second Sort.

In fine, the fourth Sort of lateral Operation is that, in which, without touching the *Urethra*, or Neck of the Bladder, the Incision is made into its Body, on one Side of the Neck. This is ascribed to Mr. *Rau*; but I am of Opinion, that the first Persons who tried it on the living Body were Mr. *Bamber* and Mr. *Cheselden*, who soon after abandon'd it; after them, Mr. *Foubert*, who endeavoured to improve it; and, in fine, myself, who am in hopes, that I have given it those Degrees of Perfection, which were essentially wanting in the Methods of those who went before me.

This fourth Sort of lateral Operation, and those Improvements which I think I have added to it, are to be the Subject of our first Remarks.

ARTICLE II.

Remarks on the fourth Sort of lateral Operation, commonly ascribed to Mr. Rau.

FRIER *Jaques's* Manner of Cutting, quite imperfect as it was, is the Source, or, at least, the occasional Cause, of all the new Methods of the lateral

ral Operation. Immediately after Frier *Jaques*, Mr. *Rau*, who was qualified to correct the Defects in the Method of the former, invented a particular one, of which he never shew'd more than the outward Part, which he could not conceal, and made a Mystery of the essential Part of the Operation. The only Particulars that have been known with Certainty, are;

1. That he made use of a grooved *Catheter* pass'd into the Bladder without being injected; and that he himself held this *Catheter* in his left Hand during the Operation.

2. That he made the outward Incision between the left *Erector* [*Penis*] and the *Accelerator* [*Urinæ*], and carried it down to near the Buttock on one Side of the *Anus*; which he did by several Strokes of the Knife.

3. That he made the inward Incision with the same Instrument, which was like the common Incision-Knife.

But, in Mr. *Rau's* Operation, we know not what Parts he cut in his inward Incision: However, from the foregoing Circumstances, I believe I can demonstrate, that this Surgeon never performed the fourth Sort of lateral Operation, of which he is said to be the Author; and that his Manner was, at most, to cut thro' the *Urethra*, the Prostate, and the Neck of the Bladder, as is done in the second and third Sorts of this Operation. [For]

First, Mr. *Rau* made use of a grooved Staff. Now, in order to cut into the Body of the Bladder, the grooved Staff is quite useless; and even the common Staff is generally of little or no Use, because the End of the

the Staff, that answers to the Body of the Bladder, is plunged very deep towards the *Pelvis*, and also very apt to slip; for which Reason Lithotomists, such as the celebrated Mr. *Chefelden*, who resolved to try this Manner of Cutting, have been obliged to inject [the Cavity of] the Bladder, that its Body might be the less subject to slip [from the Knife]; but made no Use of the Groove of the Staff, as deeming it useless. *Incisionem in sulco catheteris fieri non posse, sive necesse non esse, ut sectio in sulco fiat.* Douglas in *Heister* on the lateral Operation. But Mr. *Rau* made use of the Groove; therefore he did not make his Opening into the Bladder thro' its Body.

Secondly, This Lithotomist did not inject the Bladder; and yet his Operation was quick and safe: Wherefore, it cannot be that he cut into the Body of the Bladder. For, even with the new Staff of my Invention, which I shall by-and-by describe, and which makes a considerable Elbow forward, the Incision into the Body of the Bladder is tedious and difficult. And I can assure you, from Experience, that this same Incision with the common Staff is so difficult, that it comes near to an Impossibility; and that it is absolutely impossible to be always sure of making this Incision in one certain Place, and without fatal Mistakes, in this Method, even supposing the Bladder injected. Therefore Mr. *Rau*, who did not inject it, would have, *a fortiori*, performed an impossible Operation, and with Success too: Therefore this Surgeon did not cut into the Body of the Bladder.

Thirdly,

Thirdly, Mr. *Rau* held the Staff with his left Hand, and did the Operation with his right. Those who do the lateral Operation of the first three Sorts do not certainly find both their Hands too much for cutting into the *Urethra* and the Prostate, without injuring the neighbouring Parts: And yet it is pretended, that Mr. *Rau* could open the Body of the Bladder very exactly (an Operation which I have proved above to be impossible in his Manner); it is pretended, I say, that he did this Operation with one Hand, a Thing which is more than possible; since, in order to do it with the new Staff, which projects forward, besides the two Hands of the Operator, which are absolutely necessary, we are obliged to make an Assistant put his Finger, or an Instrument instead of it, into the *Rectum*, to keep the Gut clear of the Place of the Incision.

Fourthly, Mr. *Cheselden*, in the first Trials he made of this pretended Method of Mr. *Rau*, opened the Body of the Bladder, and found himself under a Necessity of relinquishing this Method, because putrid Ulcers were formed, in the Course of the Cure, in the cellular Membrane that surrounds the Bladder and *Rectum*. Now Dr. *Heister*, a Disciple of Mr. *Rau*, says, This Accident never happened to Mr. *Rau*; therefore he did not open the Bladder in its Body: For there is no Reason why he should avoid this Accident rather than Mr. *Cheselden*.

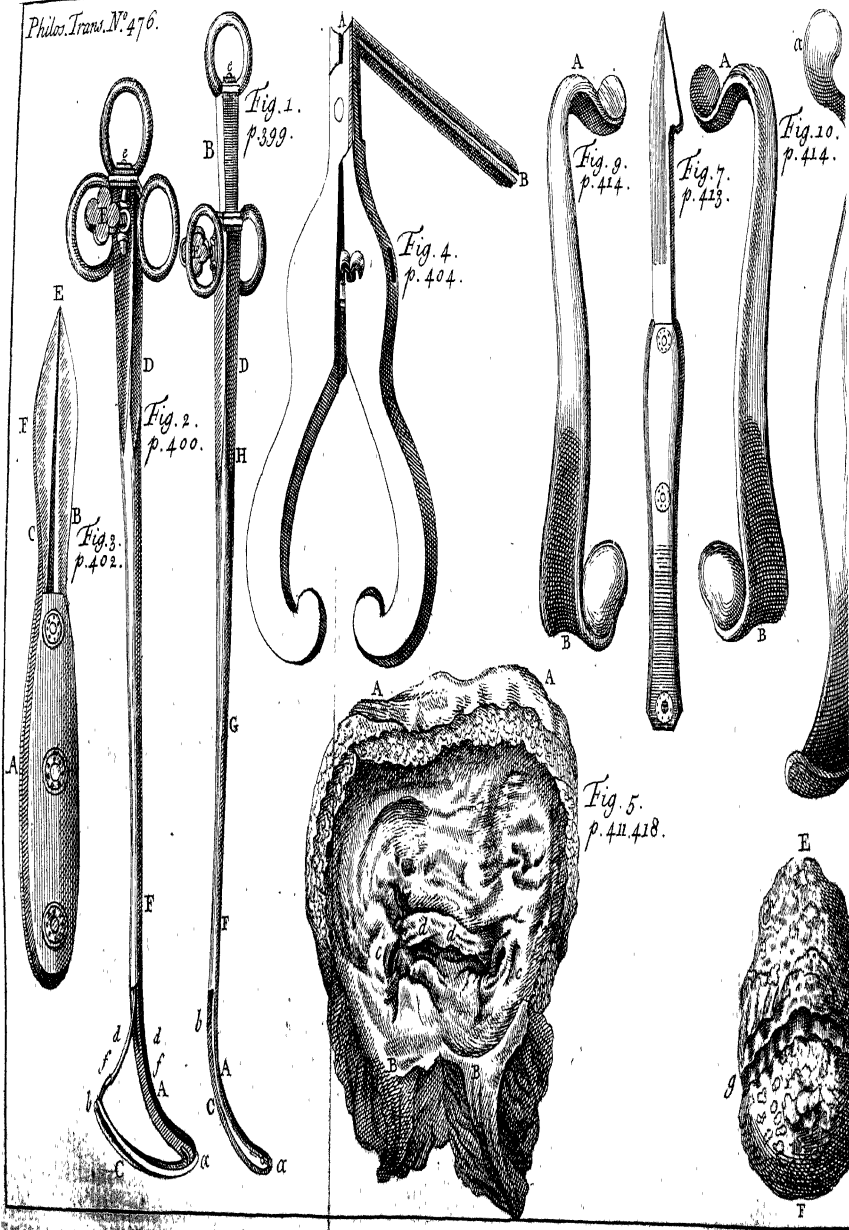
Fifthly, Mr. *Rau* made all his Incisions, the inward as well as the outward, with the same Instrument, whose Make was much like the common Cutting Knife, according to Dr. *Heister*. This last Circumstance makes me think, not only that Mr.

Rau

Rau did not cut into the Body of the Bladder; because, with so broad an Instrument, and the little Precaution he used, as we have already seen, he would never have succeeded; but likewise this broad Instrument gives me a violent Suspicion, that this Lithotomist did not so much as cut into the Neck of the Bladder; and that he only laid it open, as is the Practice in that masterly Operation with the greater *Apparatus* executed in Mr. *de la Peyronie's* Manner: For it is well known what Precaution must be used in the several lateral Operations, to remove the *Rectum* from the Prostate and the End of the *Urethra*, in order to cut these Parts without touching the Gut. Wherefore, in all the several Ways of the lateral Operation, not only the Fore-finger of the left Hand of the Operator is necessary, but also, as I have just now said, it is requisite that the Finger of an Assistant, or an Instrument introduced into the *Anus*, should contribute to keep off this same Gut.

Whether Mr. *Rau* open'd the Body of the Bladder or not, his Disciples believed he did: And as they were Witnesses to the great Success of their Master, they have not failed to conceive and give the Public an high Opinion of a Method of Cutting which opened the Body of the Bladder with all the Safety that could be expected from a true Method. The Trials made in *England* having failed of Success, it was pretty natural in *France* to think, that the Persons, who made these first Trials, had not light on the true Manner of making this Opening, so greatly cried up, and so much desired. Monsieur *Foubert*, Surgeon of *Paris*, flatter'd with these Hopes, added to the common Instruments for Cutting, the grooved

Trochart of the *Paracenthesis*, which he propos'd to thrust in between the Tuberosity of the *Os Ischium* and the *Anus*, directly into the Body of the Bladder; and on which he was afterwards to introduce a sharp Instrument of his Invention, to make a proper Wound for extracting the Stone. He made Trial of these Instruments on a dead Body; and, in fine, he cut a certain Number of Patients for several Years successively in that manner. People are divided on the Success which attended these Trials: Much has been written against it; and I have heard a great deal said in its Favour by good Judges. One Advantage, which flatter'd me in this Method, was that of its not being liable to occasion Incontinencies of Urine, nor even *Fistula's*, as I was assured; Inconveniences from which the lateral Operation is not exempt in the Case of large Stones, because this Operation attacks the Bladder in its Neck. But one Defect of Mr. *Foubert's* Method, which his very Partizans cannot help taking for an essential Inconvenience, is, that the Operator thrusts the Trochart in toward the Bladder without any thing to guide him, and, as it were, by Guess. 'Tis needless to enlarge on the fatal Consequences of this Defect; they appear at first Sight; and are sufficient to make one refuse giving even the Name of a Method to so uncertain a Way of Cutting. Nevertheless, if this Way of Cutting had otherwise great Advantages, and that one could clear it of that Blemish which overcasts it, by giving it this Guide which it wanted, and rendering its Process steady and certain, it must be allowed, that it would prove an excellent Method, a lateral operation of the fourth Sort, worthy of being put upon



TAB. IV.



Fig. 8.
p. 414.

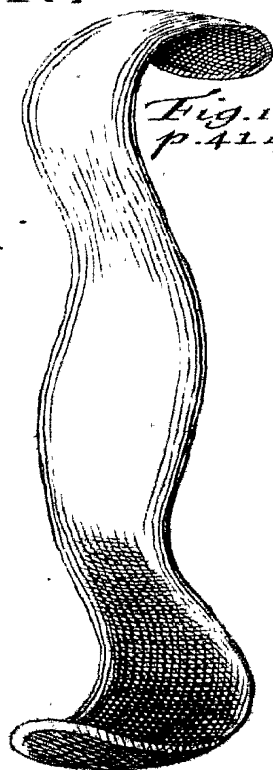


Fig. 11.
p. 414.



Fig. 6.
p. 411. 418.

upon a *Par* with, or perhaps of having the Preference of, the other three.

Such are the Reflections which I was led to make on Mr. *Foubert's* Operation, by the good Things I have heard of it, and the ill Consequences which I knew attended it. I endeavoured to find an Instrument that could fix the Incision into the Body of the Bladder to the Place intended: And here is [the Description of] that which I invented for this Purpose:

This Instrument is a Staff represented in TAB. IV. *Fig. 1.* such as it is when I pass it into the Bladder.

A, Is its crooked End, which is split lengthways into two Pieces; the concave Piece of which, *A*, is fixed, and of the same Piece with the rest of the Staff; and the convex Piece, *C*, is moveable, having its fixed Point joined by a Hinge to the End *a* of the Piece *A*, and its moveable Part jointed at *b*, with a Piece which makes the End of a strong Stilet [or Wire] that runs thro' the Centre of the Piece *B*, where it is riveted at *e*. This Piece *B*, the Wire, and the Piece *C*, are held in the Situation which the Operator puts them in, by the Screw *E*, the End of which bears against the Piece *B*. This is made of two solid Plates of Silver solder'd together; in the Middle of which a Groove has been made to lodge the Wire.

The Handle, *D*, of the Staff, is square, especially on the Inside, in order to serve as a Sheath for the Piece *B*, and give it a Firmness, which it communicates to the Wire, and to the moveable Piece *C*.

The Body, *G*, of the *Catheter* is almost entirely solid, leaving in its Centre but just enough

for the Passage of the Wire. Without this Solidity and this Narrowness of the Passage of the Wire, the *Catheter* would not be firm; and the Wire itself, liable to waver, would not run true, but would impart its Weakness, or Want of Justness, to the moveable Piece *C*, which is the Guide of this Operation.

This Body, *G*, is folder'd to the Piece *D* at *H*, making it enter square into the said Piece *D*, till it meets the Slider *B*, which I suppose intirely within the Piece *D*. The Rings are very large and strong, for the Conveniency of using it.

This *Catheter* is made of Silver, from the Rings inclusive to *F*: All the rest, together with the Wire, ought to be of the hardest Gold; because it is on this End of the *Catheter* that the greatest Stress is laid; and Silver has not Firmness enough to resist the Efforts that these Pieces must sustain. And particular Care must be taken, that all the Angles and Prominences be render'd very smooth.

I enter into these Details of the Make of the Instrument, because I have learn'd, to my Cost, that the Workmen do not think of them.

Fig. 2. shews the whole Mechanism of this *Catheter*, by representing it open, and such as it is in the Bladder while the Incision is making.

The Piece *B*, of *Fig. 1.* is here sunk in its Sheath *D*; whereby the small Style or Wire is thrust towards the crooked End of the *Catheter*, and, at the same time, pushes the End *b* of the small moveable Piece *C* towards this same Part. The *Catheter* being thus open in the Bladder, when the Operator draws the Instrument towards him, it is stopp'd by the Neck of this Organ, at the Place mark'd *dd*; and then the
Angle

Angle *b* projects about a Finger's Breadth from the Orifice of the Bladder. Yet there are some Subjects, in whom this Orifice, being very wide or relaxed, gives greater Way to the Effort made by the Widening, *dd*, of the anterior Angle of the *Catheter*; whereby it happens, that, instead of stopping this Angle at *dd*, it lets it pass thro' to *ff*, which brings the Incision so much nearer the Neck of the Bladder: Nay, I have seen in some dead Bodies, in which the Relaxation is still greater, that the Prostate was somewhat concerned in the Incision; which is no great Misfortune. But even this may be easily avoided, by taking care, in the first Incisions, to disengage the Part that answers to the projecting Angle from every thing that may hide from us the Prostate and Bladder; and then the foregoing Case becoming visible, it is easy to guard against it, by causing the projecting Angle of the *Catheter* to be push'd, or by pushing it one's Self farther into the Bladder.

This projecting Part of the *Catheter* is not seen; but it is very perceivable to the Touch, thro' the Integuments; and still more so, after they are cut through.

I must not omit observing here, that, notwithstanding all the Care I have taken to instruct the Instrument-maker in the Construction of this *Catheter*, and especially of the moveable Piece *bC*, in order to make it solid: Yet it has often proved too weak to bear the Effort of thrusting the Part forward, which we are obliged to do on one Side; so that it bent, and remain'd in the Middle, while the rest of the *Catheter* was to the left Side.

In case of this Accident, it came into my Thought to turn the *Catheter* upside-down ; so that the Angle of the moveable Piece might answer to the upper Part of the Neck of the Bladder, and stop there, while the concave and immoveable Part of the *Catheter* answer'd to the Incision, and that the very End of the *Catheter* projected at the Place where I was to open the Body of the Bladder. And, upon several Trials, I found that this Place was the very same which had before been pointed out by the Angle of the moveable Piece ; therefore, when I have one of these *Catheters*, on which I cannot depend, I make use of it in this last Manner ; and it intirely answers my Expectation ; because the fix'd Piece of these *Catheters* is always very solid, and that the Angle of the moveable Piece does its Duty as well on the upper as on the under Side of the Neck of the Bladder. It has even seem'd to me, that the End of the Instrument makes the greater Protrusion forward. In fine, this *Catheter*, being almost strait, easily assumes in the Bladder every Situation which one finds necessary to give it.

Fig. 3. represents the Incision-Knife, which I use. It is the same that I call *Urethotome* in my common lateral Operation ; excepting that here I give a greater Length to the Back.

A is the Handle, *BC* the Blade ; of which *B* is the great Edge, *C* the Back, *FE* is the little Edge. In the middle of this Blade is a Chanel, that ends with the Point of the Instrument at *E*. The little Edge *FE* must not go beyond the Point *F*, if the Operator would spare the Neck of the Bladder, when he plunges the Instrument into this Organ ; for, if it

it be made longer, it must carry the Incision as far as into the Prostate.

The Manner of performing the Operation.

HAVING placed the Patient as usual, I pass an hollow *Catheter* into the Bladder, thro' which I half-inject the Bladder; because I have found, that, as a moderate Injection renders the Operation more speedy and safe, so a complete Injection forces back toward the *Rectum* the Place appointed for the Incision, and makes the Operation laborious and dangerous.

When I have made the Injection, I draw out the hollow *Catheter*, and pass in my new Staff by half a Turn; which I make very short, upon account of its little Convexity. I push it to the Bottom of the Bladder; and when I am quite sure it is there, I give to the moveable Piece *b C*, the Situation requisite to make the Protrusion answer the Places where I intend to open the Body of the Bladder.

The Places which I have chosen in the Trials I have made of this Method are two; the first is between the two *Vesiculae seminales*, close to the left, under the Orifice of the left *Ureter*; the second is above the Orifice of the *Ureter*, and over the left *Vesicula seminalis*.

For doing the Operation pursuant to the first Intention, when my Staff is in the Bladder, I keep its Rings exactly in a horizontal Position, so that the moveable Piece *b C* may bear directly on the Line, which may be imagined to pass between the Orifices of the *Ureters*. In this Situation, I loosen the Screw which

which stop the sliding Piece *B* of *Fig. 1.* I thrust in this Piece, which opens the Staff, and makes the Elbow or Angle *b*, *Fig. 2.* I then fasten the Screw to fix the Staff thus open: I afterwards draw the Rings towards me, as if to draw back the Staff, always in a horizontal Position, till the Angle *b d a* stops me. Then, being sure that the Elbow *b* of my Staff has pass'd directly between the Orifices of the *Ureters*, and that it is a good Finger's Breadth from the Neck of the Bladder, I raise up the Handle of my Staff a little, carrying it softly toward the right Thigh, and I give to the Rings an oblique Direction approaching to a diagonal, in order to push the Elbow *b* toward the Space between the *Anus* and left *Os Ischium*. An Assistant holds the Staff in this Attitude; another passes the Fore-finger of his right Hand, or an Instrument made for this Purpose, *Fig. 4.** into the *Anus*, and pushes down the *Rectum* on the right Side. With my left Thumb I secure the Integuments, resting it on the Middle of the *Perinaeum*; and with the right Hand I make, with the Incision-Knife, *Fig. 3.* a long and deep Incision between the *Os Ischium* and the *Anus*, beginning on one Side of the Place, where ends the Incision with the greater *Apparatus*. This first Incision generally

* The this Instrument serves me here for removing the *Rectum* from the Parts destin'd for the Incision, it was not for this Use that I invented it, but to make a *Speculum Ani & Matricis*, being joined to another intirely like it, with its Angle and Groove placed on the Angle and Groove, *A B*, of this. We shall have Occasion, in another Place, to give a more ample Description of this Instrument, and its Advantages over the other Sorts of *Speculum*.

rally lays open no more than the common Integuments: Then, with the Fore-finger of my left Hand, I feel for the Elbow of my Staff; and on this Elbow I cut upward and downward; first, the *Septum*, formed by the *Elevatores Ani*; secondly, the ligamentous *Lamina* that supports these muscular Expansions, and which, without an accurate Knowledge of those Parts, may be easily mistaken for the Bladder. These Obstacles being well removed, the Elbow of my Staff becomes more and more disengaged, and the End of the Prostate and Beginning of the Bladder are laid bare. Then, being certain of the Place I am at, I feel again for the Elbow of my Staff: I set it right, if the Assistant has let it slip from its due Position, and on it I plunge the Knife into the Bladder, so as that its Point runs on the Outside, and the whole Length of this Elbow, and the great Edge *B*, *Fig. 3.* faces the posterior Part of this Organ. By this Incision I cut into the Bladder an Inch long or more, if I think proper, a Finger's Breadth from its Orifice, under and close to the left *Vesicula seminalis*, and the Orifice of the left *Ureter*.

I had caused a Groove to be made on the projecting Piece *C*, *Fig. 2.* to direct my Knife, but I found it of no Use. On the Groove of my Knife, now in the Bladder, I slide the Gorget; and then the Assistant, who held the Staff, loosens the Screw, draws the Ring of the Piece *B*, whereby the Staff is brought back to its former Shape, as in *Fig. 1.* and then he draws it out of the Bladder. The rest of the Operation is perform'd in the usual Manner.

In order to open the Bladder above the left *Vesicula seminalis* with the same Staff, as soon as it is

in the Bladder, one must turn the moveable Piece *b C* *Fig. 2.* towards the left Side of the Bladder, by giving to the Rings of the Staff such an oblique Direction, that they make an Angle of about Forty five Degrees with the horizontal Line. In this State I open the moveable Piece, and oblige it to make an Elbow exactly in the Place desired. Then I do my Operation, as above described.

The above described Operation is the Result of a great Number of Experiments made on dead Bodies; in some of which I had injected the hypogastric Artery.

These numerous Trials have constantly convinced me, that my Staff is an Instrument with which one is as sure as possible, always to open the Body of the Bladder in the Place resolved on by the Operator. For if it happens, that a Bladder either too large or too small, or some other Motive, obliges the Surgeon to make his Incision farther from, or nearer to, the Neck of this Organ, he will fulfil that Intention, by more or less pushing the Piece with the Slider *B*, and thereby causing the moveable Piece *b C* to make a greater or lesser Angle.

From Trials on dead Bodies I passed to Operations on living Subjects; and, having had some Reasons to think, that the Incision into the Body of the Bladder between and beyond the *Ureters*, was preferable to that which is practised above the left *Vesicula* [*seminalis*]. In Autumn 1741, I cut three Patients in this Way; *viz.*

John Peter Desmarest, whose Stone could not be extracted whole. He lost much Blood during the Operation, and after it; and died the 17th Day.

Peter le Grand; who died in three Months after cutting, of a sinuous *Fistula* in the *Pelvis*.

Giles Laurence; who had likewise an *Hæmorrhage*. Of three or four soft Stones which he had, the greatest Part remained, and he died the fourth Day.

As I was accustomed to good Success in our lateral Operation, I was concerned for the Accidents that happen'd in these first Trials more sensibly than another would probably have been. I publicly open'd the three above-mentioned Subjects. The Incisions of the Bladder were found to be most exactly done in the Place above specified, without hurting any of the neighbouring Parts: And it plainly appeared, that the Death of these Subjects was occasioned by the following Inconveniences attending this Method.

1. The *Hæmorrhage*, which is almost inevitable from the Depth of the Incision.

2. The Stripping of the Gut of the cellular Membrane that surrounds it; which, together with the Depth of this Denudation occasions putrid Ulcers by the Irruption of the Urine into the rest of the cellular Membrane, behind the *Septum Levator Ani*, and thence into the whole Circumference of the Bladder. Mr. *Chefelden* complained of this Accident, when he try'd this Method.

3. The Stones more difficult to be found, either with the Fingers, or the Instruments; especially when they are lodged in the right Side, and anterior Part, of the Bladder.

4. Even when the Operator has laid hold of the Stone, he finds more Difficulty in extracting it than in any other sort of the lateral Operation. I was formerly of the contrary Opinion; but Experience has since convinced me, and I have plainly seen the

Grounds of this fourth Inconvenience ; which are these :

It is not the exterior Integuments that ever hinder the Passage of the Stone ; for they yield too easily [to give any Obstacle]. The *Septum Levator Ani* is not more difficult to dilate from the Moment it has begun to be divided : Wherefore the real Obstacles to the Extraction of the Stone are, either the Bones of the *Pelvis*, the Bladder, or the Prostate.

The Bones of the *Pelvis* give an equal Obstacle to all the Sorts of lateral Operation : And even, generally speaking, in all the Methods of the *low Apparatus*, it is the same Road, the same Outlet, the same Obstacle.

The Bladder presents as great an Obstacle in the fourth Sort of the lateral Operation as in the second, or in ours. In the first, the Wound of the Bladder is made an Inch long ; in the second, the Bladder is laid bare for some Lines, and then dilated the rest of the Way. In both these the Opening is the same, when the *Forceps* is introduced. In extracting the Stone, you must in both still dilate or tear as much as the Size of the Stone requires ; and, consequently, in this respect, the Difficulty is the same.

The Prostate and Neck of the Bladder are the remaining Obstacles to the Extraction of the Stone. These Parts are divided in our Method, and they are left whole in the lateral Operation of the fourth Sort. Now it is plain, and I have experienced it in the three Operations I did, that the Neck of the Bladder and the Prostate advance, while the Stone is extracting, under the *Pubis*, and against the interosseous Ligament of the *Os Pubis*, and there form a considerable

siderable Obstacle; and indeed so considerable, that, in the Case of *John Peter Desmarest* it could not be forced by the greatest Efforts: A Circumstance which has never happened to me in the lateral Operation these ten Years past that I have used it.

5. Tho' I have had the good Fortune, in the foregoing Operations, not to hurt the *Vesiculæ seminales*, nor the *Ureter*, much less the *Rectum*; and that, with the Precautions which I have laid down in the Account of my Manner of doing the Operation, one always avoids these Accidents; yet it must be allowed, that the above-mentioned Organs are extremely near the Incision; and that in so terrible and bloody an Operation as that of Cutting for the Stone, one is not always in a Condition to make so strict an Examination as is requisite for shunning these Dangers. For which reason I reckon them as one of the Inconveniences of this Method, especially of that which opens between the two *Vesiculæ [seminales]*: And I have found it so on some dead Bodies, which I cut by an affected Negligence, without making use of that scrupulous Exactitude of which I have just now made mention. In my Opinion, the second Method I have spoken of, *to wit*, that wherein the Bladder is opened above the left *Vesicula seminalis*, is less exposed to the Hazards above mentioned, especially with our Staff. But it is subject to the other Inconveniences, and also to this additional one of Opening the Bladder in a Place thick set with [Blood] Vessels, and in particular with a very considerable *Plexus*; as is well known to those who have dissected these Parts.

In fine, I do not pretend in this Place to examine the Advantages and Inconveniences of all the different Methods.

Methods: This Detail I have reserved for a more complete Work than this Paper: At present, I only seek to lessen the Inconveniences of the general Way of Cutting by opening the Body of the Bladder; or, rather, I endeavour to improve it; and I am humbly of Opinion, that the Instrument which I have the Honour to lay before the *Society*, may contribute to this End.

REMARKS on the Operation of Cutting for the Stone.

ARTICLE III.

On the Method of Cutting by the high Operation.

WHATEVER Improvements have been made in the different Methods of Cutting for the Stone by the *low Apparatus*, there still remains in them several Inconveniences, to which the high Operation is not subject. These Advantages of the high Operation above all the other Ways, have been learnedly treated of by celebrated Authors *French* and *English*, and have not been contradicted by any one; so that it would seem as if this Operation had been abandoned, in order to run after the *lateral* Operation, pretty much as People quit an old Fashion for a new one. This being a shameful Circumstance in an Art of such Importance as Surgery is, and with respect to so serious an Operation as that of Cutting, it was at length said, in Justification of this Change, That the *low* Operation is not so general a Method as the *low*; That

That it is not practicable on Subjects of too full a Habit of Body, on indurated Bladders, which are said to be very common in Persons afflicted with the Stone.

One might say, in Answer to these Objections against the *high* Operation, that Persons troubled with the Stone are very seldom plethoric; for I do not remember to have met with one Instance of it in above fifteen Years that I am conversant with this Distemper: That most Part of the indurated Bladders become so, by the Stones being lodged many Years therein, and that such Stones are of extraordinary Size and Weight; and that, in this Case, the Bulk of the Stone alone pushes the Bladder forward enough to be able to cut by the *high* Operation; and the rather, because these Subjects are so much emaciated, that, generally speaking, one may feel the Stone above the *Pubis*, thro' the very Integuments.

This I have experienced on one *Anthony Germain*, of forty-four Years of Age, a Native of *Calais*, but residing at *Diepe*; who coming to our Hospital in order to be cut, and being dead of an accidental Fever, even before the [usual] Preparation, I cut him, by the *high* Operation, on the very Stone, and without injecting the Bladder. This Stone however was not excessively large: It is represented of half its natural Dimensions, together with the Bladder, in *Tab. IV. Fig. 5.* and *6.* and it weigh'd but eight Ounces.

But, granting that the *high* Operation is not a general Method, is there any one Method universally proper in all Cases? And even the *lateral* Operation, which I look upon as the most perfect of all the Sorts of the *low* Apparatus, does it lay Claim to this

this Universality? It must be allow'd, that it does not, whatever Attachment one has to it. True it is, that very large Stones are extracted by this Method; but it is equally true, that the Lacerations attending the Extraction of these large Stones are generally mortal, and always followed by *Fistulas*. It is a decided Point in Practice, That large Openings, whether made by Incision or Laceration, in the lower Part of the Body of the Bladder, are almost all mortal: Wherefore our Posterity may spare the Public from such murdering Experiments. Thus the Case of large Stones is one of those, wherein the *lateral* Operation becomes too fatal to venture putting it in Practice; and besides, we are not destitute of Examples to prove, that some Stones are too large to be extracted by this Method, even with all these Risques it is exposed to.

On the other hand, Experience has long since determined, that the largest Stones, even those on which the *low Apparatus* has failed, are extracted by the *high* Operation with Ease, and constant Success: This then is one Case, wherein the *high* Operation, if it be not an universal Method, is at least the only one. I think this Circumstance might have deserved more of the Attention of Lithotomists; and that, while they cultivate new Ways of Cutting by the *low Apparatus* (which are indeed useful in a great Number of Cases), they should not absolutely neglect the *high Apparatus*, which [in its Turn] is necessary in several Cases, wherein the former are either insufficient, or very dangerous. For, even supposing the Number of these Cases to be but small, the *high* Operation is not the less a necessary Supplement

plement to the other Methods for all Cases that offer.

Thus much I have learned by Experience, that is, by the ill Success of the different Methods of Cutting by the *low Apparatus* in the Case of large Stones : And therefore I am resolved to follow the *high* Operation in the said Case.

In adopting this Way of Cutting, as practised by Mr. *Douglas*, *Chefelden*, and *Morand*, I thought I might, under the Patronage of these great Men, be able to make some Improvements on it.

An Inconvenience, which always happened in doing this Operation, is, that as soon as the Knife has open'd the Bladder, the Urine or Liquor injected, which kept up its Side close to the Integuments, comes off; the Bladder sinks, and often slips from the Instrument before the Incision is made large enough; and then it is very difficult to find the Bladder, and finish the Operation, which by this means becomes tedious and painful. This Accident has happened several times.

In order to guard against it, I have thought of two Things:

First, Instead of cutting the Bladder downward, which contributes to the sinking-in of its Coats, I plunge the Knife into the Bladder behind the *Os Pubis*, and I cut it upward toward the Belly; by which means the Edge raises and supports the Coats of the Bottom of the Bladder. When I see that my Incision is large enough, I turn my Instrument quick, so that the Back may be where the Edge was: And this Back has a very smooth Shoulder, as appears in TAB. IV. *Fig. 7.* This Shoulder continues to keep

the upper Side of the Bladder close up to the Integuments. Then along the Blade of the Knife I introduce into the Bladder the Part *a* of the Instrument represented in *Fig. 8.* and placing this very easy smooth Crotchet in the room of the Back of the Knife, I give it to an Assistant to hold and keep up the Bladder close to the Integuments. This done, I put the Fingers of my left Hand very securely into the Bladder, and examine, if it be sufficiently open'd. I lengthen the Incision, if requisite; and, if the Stone presents itself to my Fingers, I draw it out, if it can be done without Difficulty; if not, I use the Instruments represented in *Fig. 9. 10. and 11.* in this manner:

By means of the first *Suspensor*, placed at the upper Angle of the Wound, I slide into the Bladder the End *B* of one of the Instruments of *Fig. 9. and 10.* I apply this second Instrument, which I call *Dilatato-Suspensor*, under one of the Lips of the Wound: I raise it up, carry it close to the Integuments, and give it to an Assistant to hold. I do the same Thing to the other Lip of the Wound with the other Instrument exactly like the foregoing. Thus the Wound of the Bladder is kept close to that of the Integuments in all its Parts; whereby the Urine cannot ouze out towards the cellular Membranes (a very common Accident in the old Way;) and it must all come thro' the exterior Opening. In the Case of little Bladders, and small Stones, the two last Instruments are sufficient for this Purpose; and then I draw out the first.

The Bladder being in this Condition, in order to extract the Stone I introduce either my Fingers, or the

the *Forceps*, or the Scoop, *Fig. 11.* which I have found, by Experience, to be much more commodious and sure than the *Forceps*. To be able to use this Instrument with the utmost Advantage, one must practise with it a little on dead Bodies: And I am bold to say, it will be found much superior to the *Forceps*; and that with it one will extract Stones with Ease and Safety, which the *Forceps* would either miss, or not draw without great Pain. The essential Reason of this Superiority is, that the Scoop takes up less Room, and that it pushes the Stone from behind, so that it can never slip back.

This Instrument, as well as the *Forceps*, is passed into the Bladder between the two Instruments that support and line the Lips of the Wound. The Passage for the *Forceps* is, as it were, mark'd out on the Back *B* of these Instruments, *Fig. 9.* and *10.* which I have designedly made lightly concave, in order to direct the *Forceps* or Scoop, and prevent their going wrong. They are in the same manner drawn out, with the Stone, between these two Concavities; and it is easy to conceive what Advantages must attend this Contrivance. The whole Stress of the Operation falls on these Concavities: All the Contusions, all the Rubbing, which these Efforts might have caused to the Lips of the Wound, and to the Bladder, bear upon these Instruments: The Shrinking of the Lips of the Bladder behind the Integuments, which is another common Consequence of these Efforts; the Tearing of the cellular Membranes, which follows this Shrinking, and makes Way for lodging the Urine, and forming purulent and mortal *Sinus's*: All these Accidents, I say, which are common in the usual *high* Operation,

and which have contributed toward disgusting some Operators, are avoided by this aforesaid Contrivance. The Instruments that extract the Stone touch the Bladder no otherwise than to lay hold of the extraneous Body : And, as soon as they have laid hold of it, they have nothing to do but with the *Dilatato-Suspensors*, which are so broad, smooth, and well polished, that they preserve the Bladder from any Hurt, dilate the Lips of the Wound as gently as the Operator thinks proper, and prepare a slippery Issue for the Stone, which must render the Operation equally expeditious and safe.

All that I have here said, has been practised upon one *John Goubert*, a Lad of seventeen Years old, of the Parish of *Plane* in *Normandy*. In searching this Lad, I judged the Stone was considerable ; and, from all the Reasons above-recited, I concluded, that he ought to be cut by the *high* Operation ; which I performed on the 23d of *May* 1742.

Instead of the Table that commonly serves to cut on, I had prepared one of those little Beds which are made by Turners. I placed my Patient so as to have his Head turn'd towards the Window, and his Feet at the opposite Part ; the Hips rais'd, the Breast low, the Head raised on a Pillow. Two Assistants, standing at his Shoulders, took hold of his Hands, and, unknown to him, threw a Ligature on each Wrist, which they fasten'd to the Bedstead : Two others did the same to his Knees ; for his Legs left hanging beyond the Bed, and held by Assistants.

Having placed myself on the Right-hand of the Patient, I did the Operation in the Manner above described, having begun by injecting the Bladder.

The

The Subject was not such as one would chuse; he had a strong Fever, and was blooded twice the Day of the Operation.

The second Day he complain'd of great Pain about the Hips; which, as he would not lie on his Belly, I attributed to the Urine got into the cellular Membranes, and beginning to hurt the Parts adjacent to the Bladder. As the Fever, and the other Accidents which the Patient complained of, were attended with a Cold over all his Body, and a Paleness of his Face, I bled him no more; but put him into a warm Bath at Eleven in the Morning. He had a Clyster given him at Four in the Afternoon, and at Night he was bathed again. He was put in on his Back with the Wound bare, that the Water of the Bath might enter in; and, when he was put to Bed, he was desired to lie on his Belly.

The third Day in the Morning, as the Symptoms were not ceased, he took a Clyster, and was bathed at Eleven o' Clock, and again at Night.

The fourth Day he was bath'd once more. He slept therein half an Hour, and the Symptoms abated.

This Method of bathing might seem strange to Practitioners; but I have used it for many Years with Success in Cases like this. The first Year that I tried it, I gave an Account of this Practice to the *Royal Academy of Sciences*.

The fifth Day my Patient was upon the mending Hand; and he lay regularly on his Belly.

The seventh Day the Accidents quite disappeared.

The fifteenth Day he was purged. The Wound was almost closed; he made Water pretty easily thro' the *Urethra*: But, what was singular, in order

to make Water thro' the Yard, he was obliged to put himself on his Belly; and when he lay on his Back, the Urine came out thro' the Wound. To prevent which, I put a common *Catheter* into the *Urethra*, which he could not bear: I changed it for one made like an S; but that also he could not bear. A Fever seized him, which made a greater Quantity of Urine pass thro' the Wound; so that I was compell'd to let him lie on his Belly, to suppress this Efflux, which, one would be apt to think, ought to be facilitated by this Situation. The Reason of this uncommon Appearance seems to be, that the Opening of the Bladder, and that of the Integuments, no longer answering to one another, the Posture on the Belly applied the Wound of the Bladder, now shrunk, and become lower, against the *Pubis*; and thereby stopp'd up the Passage of the Urine [that Way]. I allow'd him to put himself in whatever Posture agreed best with him: And, notwithstanding several Indigestions, which his Greediness of Eating was the Occasion of, he was perfectly cured.

Explanation of Fig. 5. and 6. in TAB. IV.

Fig. 5.

The Bladder of *Antony Germain* open'd throughout, and its Neck slit by two Incisions.

AA, The *Fundus* of the Bladder.

BB, The Neck of the Bladder.

cc, The Orifices of the Ureters dilated to the Breadth of an Inch.

dd, A transverse Fold, which imprinted on the Stone the circular Furrow which appears thereon.

Fig.

Fig. 6.

The Stone of *Antony Germain*, of half its natural Size, view'd by its posterior Surface, which lay on that of the Bladder represented by Fig. 5.

E, The posterior End of the Stone.

F, The anterior End answering to the Neck of the Bladder.

gg, The circular Furrow remark'd above.

N. B. The *Catbater*, TAB. IV. Fig. 1. and 2. is figured, and a short Account given of it, in the *Med. Essays*, Vol. V. p. 466. *C. M.*

XII. *Abstract of a Letter from Monsieur De Bozes, Professor of Experimental Philosophy, at the Academy of Wirtemberg, to Monsieur De Maizau. Communicated by Mr. Baker from Mr. Ellis, and translated out of the Latin by Mr. Baker.*

Read May 23. 1745. A Hollow Globe of Glass, of six or eight Inches Diameter, being swiftly turned round upon its *Axis*, by means of a large Wheel, in the Manner Mr. *Haukesby* formerly advised; and being rendered as electrical as possible by the Application of a dry woolen Cloth, or rather of a very dry Hand; if, whilst in this swift Rotation, it be brought near the End of an iron Bar, suspended by Strings of Silk that are exceedingly well dried, such an electric Power will be communicated to the the Iron, that, upon touching the other End of it with one's Finger, not only Sparks of Fire, in the usual

usual Manner, will be emitted very briskly, but even Blood will be drawn from the Finger; the Skin of which will be burst, and a Wound appear as if made by a Caustic.

2. If highly rectify'd Spirit of Wine heated in a Spoon, the ethereal Spirit of *Frobenius*, Oil of Turpentine, Sulphur, Pitch, or Resin melted, be applied to the iron Bar, instead of one's Finger, the Sparks proceeding therefrom will set it on Fire instantly.

3. A Chair being suspended by Ropes of Silk, made perfectly dry, a Man placed therein is render'd so much electrical by the Motion of the above-mentioned Globe, that, in the dark, a continual Radiance, or *Corona* of Light, appears incircling his Head, in the manner Saints are painted.

4. If several such-like Globes, or electric Tubes, are brought near the Man suspended in the Chair, the Motions of the Heart and Arteries are very sensibly increased; and if a Vein be opened under the Operation, the Blood that comes from it appears lucid like *Phosphorus*, and runs out faster than when the Man is not electrify'd.

5. Water, in like manner, spouting from an artificial Fountain suspended by silk Lines, scatters itself in luminous little Drops; and a larger Quantity of Water is thrown out, in any given Time, than when the Fountain is not made electric.

N. B. If three, four, or five Globes be employed, the Effect will be proportionably better: And Monsieur *L'Abbé Nollet* has found, that Globes or Tubes made of Glass, coloured blue with *Zaffer*, are preferable to others; for when the Glass is blue, the Experiments succeed in all Weathers;

Weathers; whereas, in damp Weather, the white Glass loses much of its electric Power.

XIII. *A Catalogue of the FIFTY PLANTS from Chelsea Garden, presented to the ROYAL SOCIETY by the Company of Apothecaries, for the Year 1743. pursuant to the Direction of Sir Hans Sloane, Bart. Med. Reg. & Soc. Reg. nuper Præs. By Joseph Miller, Apothecary, Hort. Chels. Præs. & Prælector Botan.*

Presented May 1051 **A** *Conitum hyemale. Ger. Park.*
30. 1745.

1052 *Arbutus folio serrato. C. B.*

1053 *Arum venis albis, lituris nigris maculatum.*
Hort. R. Par.

1054 *Asteroides Alpina, falicis folio. Tourn.*

1055 *Barbarea, f. B. flore simplici. Park.*

1056 *Blitum perenne: Bonus Henricus. f. B. G.*

1057 *Canella alba Off. Cort. Winterannus vulgo.*

1058 *Cardiaca, flore canescente. Amman.*

1059 *Convolvulus cœruleus minor Hispanicus. Park.*

1060 *Cyperus odoratus radice longa. C. B.*

1061 *Cyperus rotundus esculentus angustifolius. Ibid.*

1062 *Cytisus incanus, filiquis falcatis. C. B.*

1063 *Cytisus glaber foliis subrotund. pediculis brevissimis. Ibid.*

1064 *Draba filiquis donata. C. B.*

1065 *Fagopyrum vulgare erectum. Tourn.*

- 1066 Glaucium flore purpureo. *Tourn.*
 1067 Hedysarum annuum. filiquis asperis pendulis
 intortis. *Ibid.*
 1068 Helenium *Off.* Enula campana. *Park.*
 1069 Helleborus niger, flore roseo. *C. B. Off.*
 1070 Horminum luteum glutinosum. *C. B.*
 1071 Hydrophyllum *Morini. Tourn.*
 1072 Jasminum luteum *Indicum* odoratissimum.
Ferrar.
 1073 Laurus *Alexandrina. Off.*
 1074 Linaria latifolia *Dalmatica. C. B.*
 1075 Lupinus angustifolius sylvestris cœruleus elatior.
Hort. Eystet.
 1076 Lychnis coronaria vulgaris. *J. B.*
 1077 Lychnis viscosa rubra angustifolia. *C. B.*
 1078 Matricaria, foliis florum fistulosis. *Hort. R.*
Par.
 1079 Melissa *Moldavica*, flore albo. *Park.*
 1080 Myrtocistus *Pennei. Clus.*
 1081 Napus dulcis sativus. *Off.*
 1082 Onagra latifolia. *Tourn.*
 1083 Origanum *Heracleoticum*, Cunila gallinacca
Plinii. C. B.
 1084 Oenanthe stellata *Cretica, P. Alpini. Park.*
 1085 Periploca foliis oblongis. *Tourn.*
 1086 Plantago major incana. *Park.*
 1087 Polygonatum floribus ex singulis pediculis.
J. B.
 1088 Rapunculus montanus corniculatus. *Ger.*
 1089 Rubia tinctorum sativa. *Off.*
 1090 Ranunculus echinatus *Creticus. C. B.*
 1091 Sclarea. *Off.*

- 1092 *Stachys Canariensis* frutescens, verbasci folio.
Tourn.
1093 *Tamariscus latiore folio.* *Park.* Germanicus.
Tourn.
1094 *Tamariscus tenuiore folio.* *Park.* Narbonensis.
Tourn.
1095 *Tacamahac foliis ferratis.* *Plukn. Phyt.* 228.
Fig. 2.
1096 *Tanacetum vulgare luteum.* *C. B.*
1097 *Tanacetum foliis crispis.* *Ibid.*
1098 *Thlaspi Creticum* purpureum. *Park.*
1099 *Trachelium umbelliferum* *Ponæ.*
1100 *Valeriana major hortensis.* *Morison. Phu. Off.*
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XIV. *An Inquiry into the Measure of the Force of Bodies in Motion: With a Proposal of an Experimentum Crucis, to decide the Controversy about it.* By James Jurin, M. D. Fellow of the Royal College of Physicians, London, and of the Royal Society.

Read May 30.
1745.

MECHANICAL Forces may be reduced to two Sorts; one of a Body at Rest, the other of a Body in Motion.

The Force of a Body at Rest, is that of a Body lying still upon a Table, or hanging by a Rope, or supported upon a Spring, &c.

This is called by the Name of Pressure, Tension, Force, or *Vis mortua*.

The *Measure* of this Force is the Weight with which the Table is pressed, or the Rope is stretched, or the Spring is bent.

And that *Measure* being acknowledged by all Writers, there is no Dispute about this Sort of Force, notwithstanding the Diversity of Appellations by which it is called.

The Force of a Body in Motion is on all hands agreed to be a Power residing in that Body, so long as it continues its Motion; by means of which it is able to remove Obstacles lying in its Way; to lessen, destroy, or overcome, the Force of any other moving Body, which meets it in an opposite Direction; or to surmount any dead Pressure or Resistance, as Tension, Gravity, Friction, &c. for some time; but which will be lessen'd or destroy'd by such Obstacles, or by such Resistance, as lessens or destroys the Motion of the Body.

This is called moving Force, *Vis motrix*, and by some late Writers, *Vis viva*, to distinguish it from the *Vis mortua* spoken of before: And by these Appellations, however different, the same Thing is understood by all Mathematicians; namely, That Power of displacing Obstacles, withstanding opposite moving Forces, or overcoming any dead Resistance, which resides in a moving Body, and which, in Whole or in Part, continues to accompany it, so long as the Body moves.

But about the *Measure* of this Sort of Force, Mathematicians are divided into two Parties: And, in order to state the Case fairly between them, it will be necessary to shew how far the two Parties agree, and in what Point their Disagreement consists.

Both

Both Sides agree, That the *Measure* of this Force depends partly upon the Mass, or Weight, of the Body, and partly upon the Velocity with which it moves; so that, upon any Increase either of the Weight, or of the Velocity, the moving Force will become greater.

They also agree, That if the Velocity continue the same, but the Mass, or Weight of the Body, be increased in any Proportion, the moving Force is increased in the same Proportion: So that, in this Case, the *Measure* of the moving Force is the same with that of the Weight: Or, when two Bodies move with the same Velocity, if the Weight of the second be double, triple, quadruple, of that of the first, the moving Force of the second will also be double, triple, quadruple, of that of the first.

But, when two Bodies are equal, and the Velocities with which they move are different, the two Parties no longer agree about the *Measure* of the moving Force.

One Side maintains, That, when the Velocity of the second Body is double, triple, quadruple, of that of the first, the *Measure* of the moving Force of the second is also double, triple, quadruple, of that of the moving Force, being the same with that of the Velocity:

The other Side pretend, That, in the same Case, the moving Force of the second Body is four times, nine times, sixteen times, as great as that of the first; the *Measure* of the moving Force being the same with that of the Square of the Velocity.

In consequence of the Agreement in the first of these two Cases, and the Disagreement in the second, the one Side pretends, That the *Measure* of the moving Force is, in all Cases, the Product of the Weight.

Weight into the Velocity; and the other, That it is the Product of the Weight into the Square of the Velocity.

This Controversy was first started by the famous Mr. *Leibnitz*, and has been carried on by him and his Followers for near threescore Years; during which Time a great Number of Pieces have been published on both Sides of the Question, and a great Number of Experiments have been made, or proposed to be made, in order to decide it. But tho' both Parties agree in the Event of the Experiments, whether actually made, or only proposed; yet, as the Writers on each Side have found a Way of deducing from those Experiments a Conclusion suitable to their own Opinion, the Disagreement still continues as wide as ever, to the great Scandal of the Learned World.

Now, if we examine carefully into the Reason of this, and would see by what means it happens, that two opposite Conclusions are so often drawn from the same Experiment, we shall find it not so much owing to false Reasoning on either Side, (That would be easily detected, and set right), as to another Cause; namely, to their Disagreement in the Principles upon which the Reasoning is founded.

For, whereas whatever is laid down on either Side as a Principle, ought to be something all the World agrees in, at least what is admitted by the other Party; without which, all Reasoning upon it is to no Purpose; this Conduct has been so little observed in the present Dispute, that what has been offered on the one Side as an undoubted Principle or Axiom, has

has commonly been something that the opposite Party does not admit, nay, even absolutely denies.

Of this it were easy to produce a Number of Examples; but I shall content myself with two only, one taken from each Side.

Those who maintain, That the moving Force is as the Weight into the Velocity, lay down for a Principle, or Axiom, That

When two Bodies meet one another in contrary Directions, if their moving Forces be equal, neither Body will prevail over the other: And if their moving Forces be unequal, the stronger will always prevail over the weaker.

This the *Leibnitian* Party deny. They maintain,

That one of these Bodies may prevail over the other, though their moving Forces be equal: Nay, that, in many Cases, the weaker will prevail over the stronger.

It is therefore to no Purpose to alledge, That the Principle above laid down is founded on common Sense; or that it was always universally received, till this Dispute began: For, since the opposite Party now reject it, all Reasoning upon it can have no Weight with them; you must have recourse to something else.

On the other hand, those who adhere to Mr. *Leibnitz's* Sentiment, lay down for a Principle, That

Equal Effects always arise from equal Causes; provided the Causes be intirely consumed in producing those Effects.

This their Opponents do not admit, unless in the Case where those equal Effects are produced in equal Times: And therefore, till both Sides shall agree in admitting

admitting this Principle, no Argument can be drawn from it by one Party, that will be of any Service to convince the other.

But as this Principle is chiefly made use of in reasoning upon Experiments made with Springs, many of which have been produced by both Parties, in Support of their Opinions, it may be worth while more particularly to consider, What Right there is on the one Side to impose this Principle, and what Reasons may be given on the other for rejecting it.

When one End of a Spring, wholly unbent, leans against an immoveable Support, and the opposite End is struck upon by a Body in Motion, which, bending the Spring to some certain Degree, does thereby lose its whole moving Force; the moving Force of the Body may be considered as the Cause of bending the Spring; and the Bending of the Spring may be looked upon as the Effect of that Cause, which is wholly spent and consumed in producing it.

Now if two unequal Bodies, moving with unequal Velocities, strike in this manner upon two equal Springs, and each of them bend the Spring it strikes upon, exactly to the same Degree; and by so doing, the moving Force of each Body be intirely consumed; Here, say the *Leibnitian* Writers, are two equal Effects produced; for the Springs are equal, and are now equally bent; and the moving Forces, which are the Causes of those Effects, are wholly consumed in producing them; and therefore, by virtue of the Principle above laid down, those Causes must be equal; that is, the moving Forces of the two Bodies must be equal.

But

But their Antagonists will reply, That this Principle is not admitted by them, except the Times of producing those Effects are equal; and that they are not so in the Case before us: For the greater Body will take up a longer Time in producing its Effect, or in bending its Spring.

If therefore the *Leibnitian* Party pretend, That equal Effects, when produced in unequal Times, do always arise from equal Causes, they must not impose this upon their Opponents by way of Principle or Axiom, but must demonstrate it.

Till this be done, there will be Room to doubt, at least, whether the two Bodies have equal moving Forces, though they bend equal Springs to the same Degree.

For the larger and slower of these two Bodies will bend the one Spring more slowly; and, consequently, will be resisted for a longer Time, than the smaller and swifter Body will be resisted in bending the other Spring to the same Degree.

May not therefore the total Resistance of a Spring be greater, if that Resistance continues for a longer Time?

And, if the total Resistance be greater, must not the moving Force, which is destroyed and consumed by that Resistance, be also greater?

Is there not Reason then to doubt, whether the moving Forces of these two Bodies be equal, though they bend equal Springs to the same Degree?

In like manner, when a Spring, already bent to some certain Degree, does, by unbending, drive before it a Body which gives way to its Pressure, is there not Room to doubt, whether the Pressure of

the Spring may not produce a greater Effect, when that Pressure continues for a longer Time?

That Pressure may be said to produce three Effects, all of which may, if we please, be considered as different from one another.

1. The Pressure carries the Body thro' a certain Space; by which Space the Length of the bent Spring is increased, in returning to its natural Situation.

2. The Pressure gives to the Body a certain Quantity of Motion.

3. It gives the Body a certain moving Force.

Now, the first of these Effects is greater, when the Pressure acts for a longer Time. For, if the Pressure of a bent Spring, by acting for one Second upon the Body 1, carry that Body 1 thro' the Space 1; the Pressure of the same, or of an equal Spring equally bent, by acting for two Seconds upon the Body 4, will carry that Body 4 thro' the same Space 1.

Likewise the second Effect is greater, when the Pressure continues for a longer Time.

For, in the Case just now mentioned, the Body 4 will have twice the Quantity of Motion that the Body 1 has; though these two Quantities of Motion arise from the Pressure of the same, or, which is all one, of equal Springs equally bent.

Why therefore are we take it for granted, or to have it imposed upon us by way of Principle or Axiom, That the third Effect is not greater, when the Time, in which it is produced by the Pressure of the same, or equal Springs, is longer, nay, infinitely longer?

But we are told, that all the Force, which resided in the Spring, while bent, is now, upon the Unbending of the Spring, communicated to the Body moved. I ask therefore, What was that Force, or what kind of Force was that, which resided in the Spring, while bent, and without Motion? Was it a bare Pressure, or a moving Force? A *Vis mortua*, or a *Vis viva*? You must acknowledge, it was a *Vis mortua*, a bare Pressure, and nothing more. But the Force communicated to the Body, and which now resides in the Body in Motion, is a *Vis viva*, a moving Force. This therefore is not the same Force, nor a Force of the same kind, as that which resided in the bent Spring.

It will be said, however, That the Force of the bent Spring is intirely exhausted in giving the Body its moving Force. I ask therefore again, What is it I am to understand by these Words, The Force of the Spring is intirely exhausted? If the Meaning be, that the Spring could not possibly give that same Body any greater moving Force, than what it has already given, I allow it: But this does not prove, that the same Spring, bent afresh to the same Degree, or an equal Spring equally bent, cannot give a greater Force to a greater Body.

But if the Meaning of these Words be, That the Spring cannot give a greater moving Force to any Body whatsoever, I must answer, That this is taking for granted the very Point which is in Dispute. For the opposite Party pretend, That a Body of four Times the Bulk, will receive twice the moving Force in twice the Time, from the Pressure of the same Spring

in unbending itself, or, if you please, in exhausting all its Force.

It is plain, therefore, that the Followers of Mr. *Leibnitz* have no Right to say, A Body has such or such a Force, because such or such a Spring has put it in Motion by unbending itself, or can be bent by it. This is not a Position to be taken for granted, but stands in need of a Demonstration, which nobody has as yet attempted to give, at least from any uncontroverted Principle; and, till this be done, the laying down any such Position can have no other Effect, than to perplex the Controversy more and more, without Hopes of ever coming to an End of it.

For which Reason I propose to take a quite different Method in what follows, and to lay down nothing, by way of Principle or Axiom, but what is allowed of by all the World, or, at least, has never yet been contradicted *a priori*.

Axiom I.

When a bent Spring does, by unbending itself, push a Body before it, the greater the Body is, the more slowly will the Spring unbend itself.

Axiom II.

The more any Spring is bent, the greater is its Pressure.

Axiom III.

A greater Pressure produces a greater moving Force; if the Time be given.

Proposition I.

Moving Forces are not proportional to the Masses of the Bodies, and the Squares of their Velocities.

Demonstration.

Let there be two Springs, equal, and equally bent, *A* and *B*, which, by unbending themselves, push before them two unequal Bodies; the Spring *A* pushing before it the greater Body.

Now, by *Axiom I.* the Spring *A* will unbend more slowly than the other: from which it follows, that, at every Instant of the Time which the Spring *B* takes up in unbending itself, the Spring *A* will have unbent itself less than *B*, or will be more bent than *B*.

Therefore, by *Axiom II.* the Pressure of the Spring *A* will, at any Instant of that Time, be greater than the Pressure of the Spring *B* at that same Instant.

Hence, by *Axiom III.* the nascent, or infinitely small moving Force, which is produced by the Pressure of the Spring *A* in every infinitely small Part of that Time, will be greater than that produced by the Pressure of the Spring *B* in the same infinitely small Part of the Time.

Therefore, the Sum of the infinitely small moving Forces; that is to say, the whole moving Force, which is produced by the Spring *A*, during that Time, will be greater than the moving Force produced by the Spring *B* in that same Time: Or the moving Force of the greater Body will be greater than that of the lesser, at the Instant that the Spring *B*, being now wholly

wholly unbent, ceases to act any longer upon the Body it has pushed before it: And as, after that Instant, the Spring *A*, not being yet wholly unbent, continues to act upon the greater Body, the moving Force of the greater Body will still continue to increase, and consequently will more and more exceed the moving Force of the smaller Body.

But every one knows, that the Products of the Masses and Squares of the Velocities are equal in the two Bodies.

Therefore the moving Forces, which we have proved to be unequal, are not proportional to the Products of the Masses and Squares of the Velocities. Which was to be demonstrated.

To consider this in a particular Example, let us suppose the Masses of the two Bodies exposed to the Pressure of the Springs *A* and *B*, to be 4 and 1 respectively; and let the Spring *B* unbend itself, and thereby give the Body 1 its whole moving Force in one Second of Time. Then, at the End of that Second, the moving Force of the Body 4 will already exceed that of the Body 1, and will still grow greater during another Second of Time. For the Times are as the square Roots of the Masses.

Also, if the Masses be 100 and 1, the moving Force of the Body 100, will, at the End of the first Second of Time, be greater than that of the Body 1, and will continue to increase during the Space of nine other Seconds.

Corollary. When a bent Spring does, by unbending itself, drive a Body before it, the larger that Body is, the greater will be the moving Force which it receives from the Spring.

(11)

Having

Having now clearly proved, that the moving Forces are not proportional to the Squares of the Velocities, I proceed next to demonstrate, that they are proportional to the Velocities themselves: And, in order thereto, I shall, as I have hitherto done, make use of no other Principles or Axioms, than such as are admitted on both Sides, or, at least, have never yet been controverted *a priori* by either Party.

Axiom IV.

Springs of unequal Lengths, when bent alike, have equal Pressures.

We speak here of Springs equal in all Respects, except the Length only; and, by being bent alike, we mean, that they are so compressed, as that the Lengths they are now reduced to, are exactly proportional to their natural Lengths, or to the Lengths they are of when no way compressed.

In this Condition, if one be directly opposed to the other, they will mutually sustain each other's Pressure, so as to maintain a perfect *Equilibrium*: Or, if each be placed separately in a vertical Situation, they will sustain equal Weights. And in one or the other of these Cases, it is evident, that they must exercise equal Pressures.

Axiom V.

Equal Pressures in equal Times produce equal moving Forces.

Bra-

Proposition II.

Moving Forces are proportional to the Masses and Velocities jointly.

Demonstration.

Let there be two Springs, of the Lengths 1 and 2, but equal in all other Respects, and bent alike: And, in unbending themselves, let the Spring 1 drive before it a Body whose Mass is 2; and the Spring 2 another Body of the Mass 1.

Now, by *Coroll.* 11. of my general Theorem concerning the Action of Springs, these two Springs will unbend themselves exactly in the same Time; and, consequently, the Spring 2 will unbend itself with a Velocity double of that of the Spring 1: And, by *Coroll.* 12. of the same Theorem, it will give to the Body 1 a Velocity double of that, which the Body 2 will receive from the Spring 1.

Also, as the two Springs were supposed to be bent alike at first, and the Spring 2 unbends itself with a Velocity double to that of the Spring 1, it is manifest, that, during the whole Time of their Expansion, they will be always bent alike, one to the other.

Therefore, by *Axiom* IV. their Pressures will be constantly equal one to the other: And hence, by *Axiom* V. the infinitely small moving Forces produced by each of these Springs, in every infinitely small Part of Time, will be equal one to the other. Consequently, the Sums of those infinitely small moving Forces, that is, the whole moving Forces, produced by the two Springs, will be equal one to the other. And the Masses of the two Bodies being 2 and 1,
and

and their Velocities being 1 and 2 respectively, it is plain, that the moving Forces are proportional to the Masses and Velocities jointly. Which was to be demonstrated.

For the greater Facility of examining this Demonstration, we have suited it to a single Case only, and that the most simple that can be supposed: But every body will see, how easy it is to form a general one upon the same Principles.

As we do not think, that any Flaw can be found in either of the Demonstrations above laid down; and the Axioms, upon which they are founded, have never yet been disputed, as far as we know; we presume, that the *Leibnitian* Opinion about the Measure of moving Forces, is incontestably overthrown by the first Proposition, and the opposite Sentiment is as evidently established by the second.

But, if any Reader shall be of a different Opinion, we must beg Leave to propose to his Consideration the following Experiment, which we hope may justly deserve the Name of an *Experimentum Crucis*; and, as such, may put a final Period to this Controversy.

It is not new indeed, having been proposed before by myself and others; but, as the Manner, in which it was formerly offered, has given Occasion to some Objections, which, tho' not affecting the Substance of the Argument drawn from it, may yet have amused and embarrassed the less attentive Readers, I shall now propose it in such a Manner, as may obviate all those Difficulties, and, I think, will render it absolutely decisive. To me, I am sure, it will be so, since I shall immediately embrace the

Leibnitian Doctrine, if my Argument drawn from it can receive a clear and satisfactory Answer.

Experiment.

Upon an horizontal Plane, at Rest, but moveable with the least Force, suppose upon a Boat in a stagnant Water, let there be placed, between two equal Bodies, a bent Spring, by the unbending of which the two Bodies may be pushed contrary Ways.

In this Case it is evident, that the Velocities, which the two Bodies receive from the Spring, will be exactly equal, and their moving Forces will also be exactly equal; and that the Plane they move upon, and also the Boat upon which it lies, will have no Motion given them either Way. Let us call the Velocity of each Body 1, and the moving Force also 1.

Now, let us suppose the Spring to be bent afresh to the same Degree as before, and to be again placed between the two Bodies lying at Rest; then let the Plane, upon which the Spring and the Bodies lie, be carried uniformly forwards, in the Direction of the Length of the Spring, with this same Velocity 1. In this Case it is manifest, that each of the Bodies will have the Velocity 1, and the moving Force 1, both in the Direction of the *Axis* of the Spring.

During this Motion, let the Spring again unbend, and push the two Bodies contrary Ways, as before, the one forwards, the other backwards: Then the Spring will give to each of these Bodies the Velocity 1, as before, when the Plane was at Rest.

By this means the hindmost Body, or that which is pushed backwards, will have its Velocity 1, which

it

it had before by the Motion of the Plane, now intirely destroy'd, and will be absolutely at Rest.

But the Body, which is pushed forwards, will now have the Velocity 2, namely 1 from the Motion of the Plane, and 1 from the Action of the Spring.

Thus far every body agrees in what will be the Event of this Experiment.

But the Question is, What will be the moving Force of the foremost Body, or of that which is pushed forwards, and which has the Velocity 2; *viz.* 1 from the Motion of the Plane, and 1 from the Action of the Spring.

By the *Leibnitian* Doctrine, its moving Force must be 4: And, if so, it must have received the moving Force 3 from the Action of the Spring; for it had only the moving Force 1 from the Motion of the Plane.

Let us examine, whether this be possible, or reconcilable to their own Doctrine.

Their Doctrine is, That equal Springs, equally bent, will, by unbending themselves, give equal moving Forces to the Bodies they act upon, whatever those Bodies are.

We agree to this, not generally indeed; but in the Case before us, where the Bodies are of equal Masses or Weights, we agree to it.

Let us therefore imagine the bent Spring, which is placed between the two Bodies, to be divided transversly into two equal Parts. In this Case it is plain, that the two Halves of the Spring, may be considered as two intire Springs, equal, and equally bent, each of which rests at one End in *Æquilibrio* against the other Spring, and at the opposite End, presses against the Body it is to move.

Consequently, by the *Leibnitian* Doctrine, to which, in this particular Case, where the Bodies are equal, we also agree, the two Springs will give equal moving Forces to the two Bodies.

But the moving Force received by the hindmoſt Body from the hinder Spring, was undoubtedly the moving Force 1: For by that Force given it in the Direction backwards, the moving Force 1, which it had before from the Motion of the Plane in the Direction forwards, is exactly balanced and deſtroyed, the Body remaining, as was obſerved before, in abſolute Reſt.

Therefore the moving Force received by the foremoſt Body from the foremoſt Spring, was alſo the moving Force 1. And this, added to the other moving Force 1, which it had before from the Motion of the Plane, makes the moving Force 2, and not the moving Force 4, as the *Leibnitian* Philoſophers pretend.

Conſequently, that Body, which had before the Velocity 1, and the moving Force 1, and now has the Velocity 2, has alſo the moving Force 2: That is, the moving Forces are proportional to the Velocities.

*XV. A Letter from the Reverend Henry Miles
D. D. and F. R. S. to the President; con-
taining Observations of luminous Emanations
from human Bodies, and from Brutes;
with some Remarks on Electricity.*

S I R,

Read June 13. 1745. **T**HE many Experiments lately made

in Electricity, and the surprising Effects which have been produced, are Encouragements to proceed in such Trials, that by an advancing Knowledge in the Nature of these strange *Phænomena*, we may have growing Hopes some Benefit will accrue to Mankind from them, as well as Entertainment to the Curious.

I should therefore think myself very happy, could I contribute any thing towards an Improvement in this Branch of natural Knowledge, were it but by suggesting what Persons of happier Talents might make a more successful Use of, than I am able to do.

The following Accounts, which relate to an odd *Phænomenon*, that has been several times observed in some human Bodies, as well as in those of some brute Animals, I was, for a good while, backward to offer to the Consideration of those who are attentive to electric Experiments, fearing they would be thought a little foreign to the Subject; but this Apprehension has, in a great measure, been removed, by considering the Remark which the late ingenious and industrious Member of the *Royal Society* Mr.

Steph

Steph. Gray made, in consequence of the Experiment of a Boy suspended on Hair Lines; *viz.* “ Hereby
 “ we see, that Animals receive a greater Quantity of
 “ electric *Effluvia* *.”----- And, with the Leave of a
 very † learned Person, and an honourable Member of
 the same *Society*, I will add his Remark on the said
 Experiment. “ It is (says he) very observable, that
 the Communication of Electricity is much greater
 “ thro’ animal than thro’ inanimate Bodies: that is,
 “ The elastic Fluid, passing thro’ these, meets with
 “ a greater Quantity of the same Matter in them
 “ than in the other; the solid animal Fibres being
 “ more adapted to receive it.” And, a little after,
 he adds these encouraging Words;---“ Perhaps the
 “ Prosecution of such Trials upon living Creatures
 “ may, in time, make us more acquainted with the
 “ Laws and Actions of this impetuous Part (as *Hip-
 pocrates* calls it **) *viz.* the nervous Fluid in the
 “ animal Machine. *Ita res accendunt lumina
 “ rebus ††.*”

In the late Edition of the Works of the Honour-
 able Mr. *Boyle*, Vol. V. Page 646. is a Letter from
 Mr. *Clayton*, dated June 23. 1684. at *James City* in
Virginia; in which he gives Mr. *Boyle* an Account
 of a strange Accident (as he calls it); and adds, that
 he had inclosed the very Paper Colonel *Digges* gave
 him of it, under his own Hand and Name, to attest
 the Truth; and that the same was also asserted to
 him

* *Phil. Transf.* N^o. 417.
 of Poisons, *Introduct.* p. 38, 39.
 lib. vi. § 8. Τα ὀσφύρτια σφύρατα.

† Dr. *Mead* *Mechan.* Account
 Ed. 3d. ** *Hipp.* *Epidem.*
 †† *Lucret.* lib. i. ver. 1110.

him by Madam *Digges*; his Lady, Sister to the Wife of Major *Sewall*, and Daughter of the Lord *Baltimore*, to whom this Accident happened.

This Paper, very unhappily, came not to hand till after Mr. *Boyle's* Works were printed; and therefore could not be inserted with Mr. *Clayton's* Letter: But, having since met with it, I present the following exact Copy of it to you, and, if you judge fit, by your Hands to the *Royal Society*.

“ *Maryland, Anno 1683.*”

“ THERE happened, about the Month of *November*, to one Mrs. *Susanna Sewall*, Wife to
 “ Major *Nic. Sewall* of the Province abovesaid, a
 “ strange Flashing of Sparks (seem'd to be of Fire)
 “ in all the wearing Apparel she put on, and so con-
 “ tinued till *Candlemas*: And, in the Company of
 “ several, viz. Captain *John Harris*, Mr. *Edward*
 “ *Braines*, Captain *Edward Poulson*, &c. the said
 “ *Susanna* did send several of her wearing Apparel;
 “ and, when they were shaken, it would fly out in
 “ Sparks, and make a Noise much like unto Bay-
 “ leaves, when flung into the Fire; and one Spark
 “ litt on Major *Sewall's* Thumb-nail, and there con-
 “ tinued at least a Minute before it went out, with-
 “ out any Heat: All which happened in the Com-
 “ pany of

Wm. Digges.

- * “ My Lady *Baltimore*, her Mother-in-law, for
 “ some time before the Death of her Son *Cæci-*
 “ *lius Calvert*, had the like happened to her;
 “ which has made Madam *Sewall* much trou-
 “ bled at what has happened to her.”
- “ They caused Mrs. *Susanna Sewall* one Day to
 “ put on her Sister *Digges’s* Petticoat, which
 “ they had tried beforehand, and would not
 “ sparkle; but at Night, when Madam *Sewall*
 “ put it off, it would sparkle as the rest of her
 “ own Garments did.”

The celebrated *Bartholin* of *Copenhagen*, in his
 Collection of anatomical Histories that are unusual,
Century III. Hist. LXX. which he intitles *Mulier*
splendens, gives us a parallel Instance in a noble
 Lady of *Verona* in *Italy*, which, he says, he had
 from an Account of the *Phænomenon* published by
Petrus à Castro, a learned Physician of the same
 Place, in a small Treatise intituled *De Igne Lam-*
bente. There is this Circumstance not mentioned in
 Mrs. *Sewall’s* Case (tho’ perhaps it would have hap-
 pened, if Trial had been made, as well as in the Case
 of the *Italian Lady*); which I think not improper to
 mention, in *Bartholin’s* own Words, — “ *ut quo-*
 “ *tiens leviter linteo corpus tetigerit, scintillæ ex*
 “ *artubus copiose profiliant, cunctis domesticis con-*
 “ *spiciuæ, non secus ac si è silice excuterentur.*” At
 the Conclusion of this Relation he refers us to a Book
 of his, intituled, *De Luce Animalium*, for more In-
 Instances

The additional Lines are not in Colonel *Digges’s* Hand, but
 seem to be in Mr. *Clayton’s*.

stances of these lucid *Effluvia*; and says, he has there shown the Cause of them at large; but, as I have not yet got a Sight of that Book, I can say nothing further — only, that in the second Century of the Histories above-mentioned, *History* XII. he asserts, that he has prov'd, in his Book *de Luce*, &c. that Light is connatural or innate to all, as well Vegetables as Animals.

There is another Author, Dr. *Simpson*, who published a Philosophical Discourse of Fermentation, dedicated to the *Royal Society*, *Anno* 1675. who takes notice of Light proceeding from Animals, on the Friction or Pectation (as he calls it) of them; and instances in the Combing a Woman's Head, the Currying of a Horse, and the Friction of a Cat's Back; the two last of which are known to most. I cannot tell whether it be material to add, that, according to this Gentleman's Hypothesis, he would assign the Principles of Fermentation, which he supposes to be *Acidum & Sulphur*, as the Cause of these lucid *Effluvia* in Animals. His Hypothesis I may not take upon me to judge of; but I humbly apprehend, the Properties of the *Effluvia* in animal Bodies are many of them common with those produced from Glass, &c.; such as their being lucid, their Snapping, and their not being excited without some Degree of Friction, and, I presume, I may add, Electricity; for I have, by repeated Trials, found a Cat's Back to be strongly electrical, when stroak'd.

I must intreat your Candor in excusing the Errors and Imperfections you may observe in this Paper to my present weak State of Health; and I was unwilling to delay any longer a small Testimony of

my Desire to promote the Designs of the *Royal Society*; to which and its worthy *President*, I am

A faithful humble Servant,

Tooting, May 9.

1745.

Henry Miles.

P. S. In the Account of some of the earlier electrical Experiments made by Mr. Gray, *Phil. Trans.* N°. 366. we are informed, that he electrified several other Bodies, besides animal Substances, by drawing them between his Thumb and Fingers; in particular, Linnen of divers Sorts, Paper, and Fir-Shavings, which would not only be attracted to his Hand, but attract all small Bodies to them, as other electric Bodies do. Now, notwithstanding this last Circumstance of their attracting, as well as being attracted, may it not be questioned, Whether, in this Way of Trial, it appears that they are electrical Bodies, or Electrics *per se*? Is it not doubtful (since his Fingers must be excited considerably in this Experiment) whether he did not communicate Electricity to them from his Hand, rather than excite it in them? I have no doubt but that the Principle is inherent in many other Bodies besides Animal, possibly, in all Bodies whatever; But as it is allow'd, I suppose generally, that Animals have a greater Quantity of it residing in them, than other Substances, there seems Room to admit the Doubt I have mention'd, which I submit to the Consideration of such as are curious in Experiments of this kind.

XVI. *An Extract, by Mr. Paul Rolli, F. R. S. of an Italian Treatise, written by the Reverend Joseph Bianchini, a Prebend in the City of Verona; upon the Death of the Countess Cornelia Zangari & Bandi, of Cesena. To which are subjoined Accounts of the Death of Jo. Hitchell, who was burned to Death by Lightning; and of Grace Pett at Ipswich, whose Body was consumed to a Coal.*

Satius est de re ipsa querere, quam mirari.

SENECA.

Cesena, April 4. 1731.

Read June 20. 1745. **T**HE Countess *Cornelia Bandi*, in the 62d Year of her Age, was all Day as well as she used to be; but at Night was observed, when at Supper, dull and heavy. She retired, was put to Bed, where she passed three Hours and more in familiar Discourses with her Maid, and in some Prayers; at last, falling asleep, the Door was shut. In the Morning, the Maid, taking notice that her Mistress did not awake at the usual Hour, went into the Bed-chamber, and called her; but not being answer'd, doubting of some ill Accident, open'd the Window, and saw the Corpse of her Mistress in this deplorable Condition.

Four Feet Distance from the Bed there was a Heap of Ashes, two Legs untouch'd, from the Foot to the Knee, with their Stockings on; between them was the Lady's Head; whose Brains, Half of the Back-

part of the Scull, and the whole Chin, were burnt to Ashes; amongst which were found three Fingers blacken'd. All the rest was Ashes, which had this particular Quality, that they left in the Hand, when taken up, a greasy and stinking Moisture.

The Air in the Room was also observed cumber'd with Soot floating in it: A small Oil-Lamp on the Floor was cover'd with Ashes, but no Oil in it. Two Candles in Candlesticks upon a Table stood upright; the Cotton was left in both, but the Tallow was gone and vanished. Somewhat of Moisture was about the Feet of the Candlesticks. The Bed receiv'd no Damage; the Blankets and Sheets were only rais'd on one Side, as when a Person rises up from it, or goes in: The whole Furniture, as well as the Bed, was spread over with moist and ash colour Soot, which had penetrated into the Chest-of-drawers, even to foul the Linnens: Nay the Soot was also gone into a neighbouring Kitchen, and hung on the Walls, Moveables, and Utensils of it. From the Pantry a Piece of Bread cover'd with that Soot, and grown black, was given to several Dogs, all which refused to eat it. In the Room above it was moreover taken notice, that from the lower Part of the Windows trickled down a greasy, loathsome, yellowish Liquor; and thereabout they smelt a Stink, without knowing of what; and saw the Soot fly around.

It was remarkable, that the Floor of the Chamber was so thick smear'd with a glutinous Moisture, that it could not be taken off; and the Stink spread more and more through the other Chambers.

REMARKS.

IT is impossible, that, by any Accident, the Lamp should have caused such a Conflagration.

There is no Room to suppose any supernatural Cause.

The likeliest Cause then is a Flash of Lightning; which, according to the most common Opinion, being but a sulphureous and nitrous Exhalation from the Earth, having been kindled in the Air, did penetrate either thro' the Chimney, or thro' the Chinks of the Windows, and did the Operation. All the above mentioned Effects prove the Assertion; for those remaining foul Particles are the grossest Parts of the *Fulmen*, either burnt to Ashes, or thickened into a viscous bituminous Matter. Hence no Wonder the Dogs would not eat of the Bread, because of the Bitterness of the Soot, and Stink of the Sulphur that lodged on it. The impalpable Ashes of the Lady's Corpse are also a Demonstration; for nothing but a *Fulmen* could produce such an Effect.

They say that there was not any Noise; but may be there was, and they heard it not, being in a sound Sleep: Besides, there have been seen Lightnings and *Fulmina* without Noise; as one may very often observe.

THIS is the whole *Narration*; after which I think proper to place what is said in the Preface relating to it.

IN the *Acta Medica & Philosophica Hafniensia*, published by the celebrated *Thomas Barolin*, 1673.
Vol

Vol. II. p. 211. n. 118. one may see such another Accident related in these very Words.

“ A poor Woman at *Paris* used to drink Spirit of Wine plentifully for the Space of three Years, so as to take nothing else ! Her Body contracted such a combustible Disposition, that one Night she, lying down on a Straw-Couch, was all burned to Ashes and Smoke, except the Scull, and the Extremities of her Fingers.”

John Henry Cohausen relates this Fact in a Book printed at *Amsterdam* 1717, intituled, *Lumen novum Phosphoris accensum*; and in the first Part, p. 92. relates also, “ That a *Polish* Gentleman, in the Time of the Queen *Bona Sforza*, having drank two Dishes of a Liquor called Brandy-Wine, vomited Flames, and was burnt by them.”

REMARKS.

SUCH an Effect was not produced by the Light of the Oil-Lamp, or of any Candles; because common Fire, even in a Pile, does not consume a Body to such a Degree; and would have besides spread itself to the Goods of the Chamber, more combustible than a human Body. It seems also, that it was not what is commonly taken for a *Fulmen*; for there was not left in the Place any sulphureous and nitrous Smell: There did not appear any blackish Tracks on the Walls; all Signs of the *Fulmina*, as they have been remarked by the exactest Observer of *Phænomena*, the celebrated Mr. *Boyle*: But if it was not a real *Fulmen*, it was certainly of such a Nature.

Some thought, that in the Ground under the Room there had been a Mine of Sulphur: Which, grained;

what then ? I know, by Experience, that in the very Mines of Sulphur have perished some of the Miners, but only by Suffocations caused by some sudden copious Exhalation of kindled Sulphur ; and never by having been burnt to Ashes. The Miners have informed me on the Spot, that those of them who perished, have been only choaked by a strong nitrous and sulphureous *Effluviu*m ; but none of them by having been set on Fire.

The Author relates, That, going once out of Curiosity into a Sulphur-Mine by *Montefiascone*, when near the Place from whence the Miners digg'd out the Sulphur, he was advised by one of them, who was carrying out his Load, not to go farther ; for, either the Smell, or some sudden Exhalation, might have done him great Injury ; and when got again in the open Air, told him, a few Days before three of his Fellow Miners fell stone-dead, while they were at Work, by a violent Suffocation, caused by a strong Exhalation of bituminous Smoke, which burst violently from the Place where they were digging ; which Misfortune was too frequent in such Mines ; but he never heard nor saw, that any of them had been burnt.

Thence it is concluded, that if the *Fulmina* have such an Effect, the *Incendium* proceeds originally from their nitrous, and not from their sulphureous Parts ; because the Air, very closely imprison'd in the Nitre, and not in the Sulphur, either by its own Elasticity, or by some other Agent being put in Agitation, produces the Flame of the *Fulmen* ; which burns and consumes any thing to Ashes.

I have

I have, says he, seen the famous Sulphur-Spring, a Mile distant from *Pozzoli*, mentioned by *Petronius Arbitr*: At the lower End of the Plain there is a Pit of liquid Sulphur, whose boiling *Æstuations* rise ten or twelve Feet. Its liquid Matter consumes the Flesh of any Corpse, but does not affect the Bones in the least. In our Case the very Bones were burnt to Ashes; and still the Pavement was not damaged. No sulphureous Smell remained in the Chamber.

All this he advances, to oppose the Opinion of an Academician at *Ravenna*, who insisted, That underneath that Chamber must be a sulphureous Mine. Which Opinion he founds on this, That, in the very House, in a Room near that the Lady was burnt in, there was set on Fire a good Quantity of Hemp, and could not be found out by whom; as also, that, all on a sudden, Part of the Palace had fallen, and not by any Earthquake; so that one might conjecture all this to be Effects of the sulphureous Mine underground; Which is not proved by those Assertions. Nay, on the contrary, if there was a Mine of Sulphur, one should smell the Stink of it in those dull Days, when the nauseous South Wind blows; the Sulphur Mines then stinking at a great Distance: Besides, the Effects of Sulphur are not to reduce a Body into impalpable Ashes.

The AUTHOR'S Opinion.

THE Fire was caused in the Entrails of the Body by inflamed *Effluvia* of her Blood, by Juices and Fermentations in the Stomach; by the many combustible Matters which are abundant in living Bodies for the Support of Life; and, finally, by the fiery Evaporations which

which exhale from the Settlings of Spirit of Wine, Brandies, and other hot Liquors in the *Tunica villosa* of the Stomach, and other *adipose* or fat Membranes; within which (as Chymists observe) those Spirits ingender a kind of Camphire; which, in the Night-time, in Sleep, by a full Breathing and Respiration, are put in a stronger Motion, and, consequently, more apt to be set afire.

P R O O F S.

FAT is an oily Liquid separated from the Blood by the Glands of the *Membrana adiposa*; and it is of an easily combustible Nature, as common Experience shews.

Our Blood is of such a Nature; as also our Lymph and Bile: All which, when dry'd by Art, flame like Spirit of Wine at the Approach of the least Fire, and burn away into Ashes. [*Observ. 171. in the Ephemeris of Germany, Anno X.*]

Such a Drying-up of Matters may be caused in our Body by drinking rectified Brandy, and strong Wines; as Monsieur *Litre* observed in the Dissection of a Woman 45 Years old, in the History of the *Royal Academy of Sciences*, 1706. p. 23. Which Effect may oftener happen, if the Spirit of Wine has any Mixrure of Camphire: For that Liquor is but a sublimated Oil, whose sulphureous Particles, being attenuated by the Fermentation, when separated from fixed and salt Matters, are easily put in Motion, and, rolling thro' the Air, become Flame and Fire.

Besides, altho' the Salts which are in living and vegetable Creatures are not naturally inclined to kin-

dle; nevertheless they often contribute to it, particularly, when there is join'd some strong boiling Fermentation. It is from such a Cause, that we know how the Mixture of two Liquors, altho' cold to the Touch, produces a flaming Fire.

Becher was the first Discoverer of this marvellous *Phænomenon*, by mixing Oil of Vitriol with that of Turpentine. *Borrichius* afterwards did the same, by mixing Oil of Turpentine with *Aqua fortis*; and at last Monsieur *Tournefort*, by joining Spirit of Nitre with the Oil of *Sassafras*; and Monsieur *Homburg* with this acid Spirit, together with the Oil and Quintessences of all the aromatic *Indian* Herbs: Nay, Mr. *Homburg* asserts, that with a certain cold Water Cannons were fired, *Anno* 1710. in the abovesaid History of the *Academy of Sciences*, p. 66.

It is out of Question, how, by a strong Fermentation, Magazines of Gunpowder, Barns, Paper-Mills, and Haycocks, have been set afire.

The acid Particles in our Bodies are much united with the Fat and oily Parts; nay, all our Limbs abound with Oil and Acid. What Wonder then, if they may kindle? as Mr. *Homburg* well observes, in the aforesaid History, 1712. 1717. from p. 13. to 31. where he takes notice, that all our Limbs have abundance of fetid Oil, and volatile Salt, and therefore easily combustible.

We ought not to omit how the Teeth are form'd by so many short Tubes, the Bones by long ones, and easier therefore to be set on Fire. *Malpighi* observed also, that the Bones contain a fat oily Matter.

Besides all this, we know that the *sebaceous Glands* are spread all over the Body; and that an oily Moisture,

ure, with now-and-then a nitrous sulphureous Smell, perspires from our Skin; to which Dr. *Blancard* ascribes the whole Circulation.

Abundance of combustible Matter, shut up in a great Number of Cells, lies in the *Omentum*.

There is further to be consider'd the vast Quantity of *Effluvia* that emanate from our Bodies. *Sanctorius* observed, that, of eight Pounds of Food and Drink in a Day, there is an insensible Perspiration of about five; computing with them those *Effluvia* which go out of the Mouth by Breathing, and which might be gathered in Drops on a Looking-glass [*Señt. 1. Aphor. 6.*]. As also, that, in the Space of one Night, it is customary to discharge about 16 Ounces of Urine, 4 of concocted Excrements by Stool, and 40 and more by Perspiration [*Aphor. 59.*]. He teaches also, that Numbness is an Effect of too much internal Heat, by which is prevented such an insensible Transpiration; as we will shew in this very Case.

On this Supposition I say, that the *Effluvia* of such an insensible Transpiration are an inflammable Mine, easily apt to kindle, whenever a Friction, be it ever so small, puts them in quick Motion, and increases their Velocity.

We acknowledge the Discovery of this evident Truth from Mr. *Hauksbee*, *F. R. S.* in the Experiment so much known of the *Glass Globe*, p. 30. to which I refer the Reader. I saw this Experiment at *Rome*; and altho' it seems that the Light be only *Phosphorus* produced by the *Effluvia* coming out of the Hand, and of the Glass, it may nevertheless occasion further Meditation on the present Case.

The Friction of the Palms of our Hands, or of any other Parts of our Body, may produce those Fires commonly called *Ignes Lambentes*.

We learn of *Eusebius Nierembergius*, that such was the Property of all the Limbs of the Father of *Theodoricus*: such were those of *Charles Gonzaga*, Duke of *Mantua*, as the celebrated *Bartolin* took notice of. By the Testimony of *John Fabri, M. D.* a noted Philosopher, who saw it, Sparkles of Light flash'd out of the Head of a Woman, while she comb'd her Hair. *Scaliger* relates the same of another Woman. *Cardanus*, of a *Carmelite* Monk, whose Head continued 13 Years to flash out Sparkles, every time he tossed his Cowl on his Shoulders. *Ezekiel à Castro, M. D.* a famous Jew, and afterwards a *Christian*, wrote a little Treatise, intituled, *Ignis lambens*; on the Occasion that the Countess *Cassandra Buri*, of *Verona*, when she rubb'd her Arms with a Cambrick Handkerchief, all the Skin shined with a very bright Light. *Eusebius* relates the same of *Maximus Aquilanus*. *Licetus* heard say by his Father, that he saw the same Quality on *Francis Guido*, a Civilian; and that he himself knew *Antony Cianfo*, a Bookseller in *Pisa*, who, when he shifted, shined all over with great Brightness. *Libavius* relates the same of a Youth; and *Cardanus* of a Friend of his; saying, that, when he shifted, clear Sparkles of Fire shot forth of his Body. Father *Kircher*, a Jesuit, relates, how he, going in Company into a subterranean Grotto at *Rome*, saw Sparkles of Fire evaporate from the Heads of his Companions, grown warm by walking. Father *Alphonso d'Ovale* as Eye-witness on the highest Mountains of *Peru* and

and *Chili*, how both Men and Beasts there seem shining with the brightest Light from Top to Toe.

These Flames seem harmless, but it is only for want of proper Fuel. *Peter Bovisteau* asserts, that such Sparkles reduced to Ashes the Hair of a young Man. *John de Viana*, in his Treatise intitled, *De Peste Malagensi*, p. 46. relates how the Wife of Doctor *Freilas*, Physician to Cardinal *de Royas*, Archbishop of *Toledo*, sent forth naturally, by Perspiration, a fiery Matter, of such a Nature, that if the Roller that she wore over her Shift was taken from her, and exposed to the cold Air, it immediately was kindled, and shot forth like Grains of Gunpowder *.

After all this I say, that a feverish Fermentation, or a very strong Motion of combustible Matters, may rise in the Womb of a Woman, with such an igneous Strength that can reduce to Ashes the Bones, and burn the Flesh. Two such Cases are known, one in the *Acta Medica Philosophica & Hafniensia*, Anno 1673. by the Observation of *Matt. Jacobei*; and the other in *M. Marcell. Donato, de Medic. Hist. Mirab. lib. IV. cap. 25*, p. 248. & *lib. VII. Cosmog. c. 1.* of *Cornel. Gemma*.

I say also, that the Bile, which is a necessary Juice for our Digestion, was observed by *Peter Borelli*, that, being vomited up by a Man, it boiled like *Aqua fortis*. [*Centur. II. Obs. 1. p. 109.*].

Besides,

* *Pet. Borelli* gives an Instance of such *Effluvia* not only producing Light, but likewise Fire. See his *Obs. Cent. II. Obs. 75. p. 174.* where he says, That there was a certain Peasant, whose Linen, hempen Thread, &c. if laid up in Boxes, tho' wet, or hung upon Sticks in the Air, did soon take Fire; which hath been seen by a great Number of Spectators.

Besides, very strong Fires may be kindled in our Bodies, as well as in other Animals of an hot Temperament, not only by Nature, but also by Art; which, being able to kill, will serve for a better Proof of my Argument. It is necessary, for clearer Instance thereof, to read the 77th Observation of *John Pisano*, in the *German Ephemerides*, printed in *Lipsia* 1670.

Tie the upper Orifice of the Stomach of an Animal with a String; tie also its lower Orifice; then cut it out above and below the Ligatures, and press it with both Hands, so that it swells up in one Side; which done, let the left Hand keep it so that the swell'd Part may not subside; and, with the right, having first, at an Inch Distance, placed a Candle, open it quick with an anatomical Knife, and you will see a Flame there conceiv'd, coming out in a few Seconds of Time: And such a Flame may, by the Curious, be perceived not only in the Stomach, but also in the Intestines. The first Discoverer of this was *Andrew Vulparius*, Anatomy-Professor at *Bologna* in *Italy* 1669. Thus you see, that a quick and violent Agitation of Spirits, or a Fermentation of Juices in the Stomach, produces a visible Flame. *Pisano* was an Eye-witness of the above-related Operation.

In the *German Ephemerides*, anno X. p. 53. of the Continuation by *John Christopher Sturmius*, one may read, That often, in the Northmost Countries, Flames evaporate from the Stomachs of those who drink strong Liquors plentifully. About 17 Years ago, says the Author, three Noblemen of *Curland*, whose Names, for Decency-sake, I will not publish, drank, by Emulation, strong Liquors; and two of them

them died scorch'd and suffocated by a Flame forcing itself from the Stomach.

The most celebrated *Borelli* relates how he was told, that a Woman vomited Flames in the Point of Death: You may read, says he, in *Bartolinus de Luce*, and in *Eusebius Nierembergensis* his History *Nat. peregrin.* how such Accidents did often happen in great Drinkers of Wine and Brandy: Where is related also, how Fire came out from the privy Parts of a Woman.

My Lord *Bacon*, in his *Nat. Univ. Hist.* assures, he had seen a Woman's Belly sparkling like Fire; and truly such Flames would often rise in us, if the natural Moisture did not quench them; as *Lucretius* observes, from *Verse* 868. *Lib.* IV. and *Verse* 1065. *Lib.* VI. Moreover, *Marcellus Donatus*, in his *Mirab. Hist. Medic. Lib. VI. Cap. 4.* intituled, *Of a new Distemper*, says, *Albertus Krantzzius, Lib. V.* of his *Saxon History*, That, in the Time of *Godfrey of Bologne* his Christian War, in the Territory of *Niverwa* or *Nivers*, People were burning of invisible Fire in their Entrails, and some had cut off a Foot or an Hand where the Burning began, that it should not go further. *Ezekiel de Castro*, in the above-said Work of his, of *Lambent Fire*, relates the famous Instance of *Alexandrinus Megetius*, a Physician, who, from the *Vertebra* of the *Coxa*, after great Pain, relates how Fire came out, which burn'd the Eyes, as *Simplicius* and *Philaseus*, Eye-witnesses, did attest.

After all these Instances, what Wonder is there in the Case of our old Lady? Her Dulness before going to Bed was an Effect of too much Heat concentrated

trated in her Breast, which hindered the Perspiration through the Pores of her Body; which is calculated to about 40 Ounces *per* Night. Her Ashes, found at four Feet Distance from her Bed, are a plain Argument, that she, by natural Instinct, rose up to cool her Heat, and perhaps was going to open a Window.

The learned Marquis *Scipio Maffei* was told by Count *Atimis* of *Gorizia*, who passed through *Cesena* a few Days after the Accident, that he heard say there, how the old Lady was used, when she felt herself indisposed, to bathe all her Body with camphorated Spirit of Wine; and she did it perhaps that very Night. This is not a Circumstance of any Moment; for the best Opinion is that of the internal Heat and Fire; which, by having been kindled in the Entrails, naturally tended upwards; finding the Way easier, and the Matter more unctuous and combustible, left the Legs untouch'd; which may have been saved also, by remaining cut off at the Combustion of the Tendons, where they join with the Knees. The Thighs were too near the Origin of the Fire, and therefore were also burnt by it; which was certainly increased by the Urine and Excrements, a very combustible Matter, as one may see by its *Phosphorus*. *Galenus* (*Class.* 1. *Lib.* III. *de Temperam.*) says, That the Dung of a Dove was sufficient to set Fire to a whole House. And the learned Father *Casati*, a Jesuit, in his *Phys. Dissert.* Part 2. p. 48. relates to have heard a worthy Gentleman say, That, from great Quantities of the Dung of Doves, Flights of which used, for many Years, nay, Ages, to build under the Roof of the great Church of *Pisa*, sprung originally the Fire which

which consumed the said Church *. After all this, the Author concludes, That to be sure the Lady was burnt to Ashes standing; drawing the Consequence from her Skull fallen perpendicular between her Legs; and that the Back-part of her Head had been damaged more than the Fore-part, was because of her Hair, and of the Nerves, whose principal Seat lies there: and besides, because in the Face there were many Places open, out of which the Flames might pass; as it happened in the Time of the *Roman* Consuls *T. Gracchus* and *M. Juventius*, when a Flame came out of a Bull's Mouth, without hurting the Beast, by not finding any Resistance to its Way.

Extract of a Pamphlet, intituled, "Fire from Heaven burning the Body of one John Hitchell, of Holneburst, within the Parish of Christ-Church, in the County of Southampton, the 26th of June, 1613:" By John Hilliard. Printed at London, 1613.

THE Manner of the Accident is as followeth:
He (*John Hitchell*), having been, on *Saturday* the 26th of *June* last, at Work at the House of one *John Deane* of *Parly Court*, where he truly
and

* Which Effect is confirmed by *Galen, lib. II. de Morb. Diff. cap. 2.* where he says, That he hath seen Pigeons Dung take Fire, when it was become rotten.

and painfully laboured at his Trade, being a Carpenter, and having ended his Day's Work, went home to his House; and, after his coming home, betook himself to his Rest; and, being in Bed with his Wife and Child, in the Deep of the Night, the Lightning came on so fiercely, that an old Woman, named *Agnes Russell*, Mother to the Wife of the said *John Hitchell*, having received a terrible Blow on her Check (by what means I know not), was therewith awakened, and cry'd to the said *John Hitchell* and his Wife to help her: But they not answering, the poor old Woman started out of her Bed, and went unto the Bed where they lay, and awakened her Daughter, who was, upon the sudden, most lamentably burnt all on one Side of her, and her Husband and Child dead by her Side. Yet, nevertheless his poor Wife, when she saw her Husband and Child had thus strangely finished their Days, she (as it seemeth) thought not so much of the Hurt she had received herself, as she was careful to have preserved the Life of her Husband, if by any means possibly she could; and therefore (notwithstanding all her grievous Wounds) she dragg'd him out of the Bed into the Street; and there, by reason of the Vehemency of the Fire, she was inforced, to her no small Grief, to forsake him; where he lay burning upon the Ground for the Space of three Days after, or thereabouts. Not that there was any Appearance of Fire outwardly to be seen on him, but only a kind of Smoke ascending upwards from his Carcase, until it was consumed to Ashes, except only some small Show of Part of his Bones, which were cast into a Pit made by the Place.

An Extract of the Minutes of the Royal Society, of Nov. 8. and 15. 1744. concerning the Woman at Ipswich, who was found burnt to Ashes on April 10. preceding.

THE first Account of this extraordinary Accident was in a Letter from Mr. *R. Love* to his Brother Mr. *Geo. Love*, Apothecary at *Westminster*, dated *Ipswich*, June 28. 1744. which was laid before the Society by the President on Nov. 8. following; wherein Mr. *Love* says, " That it appeared, upon the Coroner's Inquest concerning the Death of this Woman (at which he attended), that she, having gone up stairs with her Daughter to Bed, went down again from her, half undress'd; and that, the next Morning early, her Body was found quite burnt, lying upon the Brick-Hearth in the Kitchen, where no Fire had been, with the Candlestick standing by her, and the Candle burnt out, with which she had lighted herself down; and that the Daughter could assign no Reason for her going down, unless it were to smook a Pipe; but said she was not addicted to drink *Gin*. The Jury brought it in Accidental Death."

Nov. 15. Dr. *Lobb* communicated two Letters concerning the same Woman; one from the Reverend Mr. *Notcutt* at *Ipswich*, to the Reverend Mr. *Gibbons*; this dated July 25. 1744. and the other from the said Mr. *Gibbons* to a Friend, dated Sept. 2. following.

They both agree in all the material Circumstances relating to the Fact; both giving their Relations from the Mouths of the Eye-witnesses, who viewed the

Body when it was first found burning ; particularly Mr. *Gibbons* from the Woman's own Daughter, and from 2 other Persons living in the same House, whose Names are *Boyden*. The Case was this ; One *Grace Pett*, a Fisherman's Wife, of the Parish of *St. Clement's* in *Ipswich*, aged about 60, had a Custom, for several Years past, of going down stairs every Night, after she was half undress'd, to smoak a Pipe, or on some other private Occasion. The Daughter, who lay with her, fell asleep, and did not miss her Mother, till she awaked early in the Morning, *April* 10. 1744. when, dressing herself, and going down-stairs, she found her Mother's Body lying on the right Side, with her Head against the Grate, and extended over the Hearth, with her Legs on the Deal-Floor, and appearing like a Block of Wood burning with a glowing Fire without Flame ; upon which quenching it with two Bowls of Water, the Smother and Stench thereof almost stifled the Neighbours, whom her Cries had brought in ; the Trunk of the Body was in a manner burnt to Ashes, and appeared like an Heap of Charcoal cover'd with white Ashes ; the Head, Arms, Legs and Thighs were also very much burnt.

It was said, that the Woman had drank very plentifully of Gin over-night, on the Occasion of a Merry-making, on account of a Daughter who was lately come home from *Gibraltar*. But the Difficulty is to account for the Fire by which she was burnt ; since there was none in the Grate, and the Candle was burnt out in the Socket of the Candlestick, which stood by her ; and a Child's Cloaths on one Side of her, and a Paper Screen on the other, were both untouch'd : And altho' the melting of the Grease had so penetrated into the Hearth, as not to be scour'd out, yet they observed,

observed, that the Deal-Floor was neither singed nor discolour'd; and the Manner of the Fire burning in her Body is described as the working of some inward Cause, and not from the burning of her Cloaths, which were only a Cotton Gown and upper Petticoat.

XVII. *An Account of a Quadruped brought from Bengal, and now to be seen in London: Presented by James Parsons, M. D. & F.R.S.*

Read June 27. 1745. **B**EING always desirous of laying before this Learned Society whatsoever appears to me new and curious, I embraced the present Opportunity of viewing and describing this Creature, which I cannot find mentioned by any Natural Historian, nor any Figure exhibited, in the least, like it. Nor is it indeed to be wonder'd at, since the Beast was brought to *Bengal*, from a very remote Part of the *Mogul's* Dominions; insomuch that no Person at *Bengal* had the least Knowledge of him.

The only Hint that seems to point at this Creature, is that mentioned by *John Albert de Mandelsloe*, in his *Voyages thro' the Indies*, which are published in *HARRIS's Complete Collection of Voyages and Travels*, N^o. 52. p. 775. where he says, that, among the Horses in the Stables of the *Viceroy* of *Goa*, he saw " a Beast called a *Biggel*, a Creature " much about the Colour and Bigness of a Rain- " deer: Its Head like that of a Horse; its Main like " that.

“ that of an Ass, with black cloven Feet, and two
“ black Horns on his Head.”

This is the Whole of his Account, which is so imperfect, that it can hardly be thought absolutely to mean this very Beast before us. The following Description and Drawing I hope will be found pretty exact; and may serve to shew with which *Genus* of Quadrupeds he may be classed, and, consequently, what proper Name may be given him; which I submit to this learned *Society* to determine.

The Creature is a Male, having the *Penis* and Testicles like those of a Deer; but, as the *Penis* does not come very forward, it cannot be seen in a side View of the Animal.

The Head is formed like that of a Deer, with a rhomboidal Spot of black Hair on his Forehead; his Ears are dark without, and yellowish within, with dark Spots toward their Edges; and the Horns rise about seven Inches high, bending forwards; which is very particular, because those of all other horned Animals are directed sideways or backward, except the Brow-Antlers of some Kinds of Deer. He keeps his Ears in continual Motion, which is an Action common with Deer, and butts with his Horns as they do. His Eyes are black and lively, and the *Rictus-Oris* is long.

His Neck bends forward like the Deer Kind, but is thick and strong, somewhat resembling that of a Male Deer in rutting Time. His Mane is thin of Hair like that of an Ass, and on the convex Part of his Neck forward he has a Tuft of black Hair.

His Shoulders are thick, and his Breast pretty broad and strong, from which a Piece of loose Skin hangs like the *Dewlap* of a Cow.

His

His Legs are slender, with cloven Hoofs like those of a Deer.

His Back rises, directly over his Shoulder, pretty high, upon which the Mane, continued from his Neck, ends in a Tuft of Hair.

From the Back of this *Bunch* or Rising, his posterior Parts resemble those of an Ass, having a Tail like that of the Ass, only it is flat on the Side next the Animal, and convex on the Back. It is about two-and-twenty Inches long, and ends with some long Hairs.

He is of a light Ash-Colour, having a smooth Coat of short Hairs, which grow darker, inclining towards a black, upon some Parts of his Limbs. He has some White under his Belly towards his Breast, and under his Tail, with white Testicles.

He feeds on Hay, Grass, or any kind of Greens; and, being tried whether he would eat raw Flesh, refused it. His Keeper says, when he lies down he chews the Cud; and his Excrements are like those of a Deer.

He is about twelve Hands high to the Top of the Bunch in his Back.

His Keeper says, he never lies down on either Side, but directly upon his Limbs like the *Camel*, and that he rises as suddenly as that Beast.

There is something very particular in his Voice, which imitates the creaking Noise of a Child's Rattle, or the Croaking of some Birds, rather than the Voice of any Quadruped except the Deer, who, think, exhibits something like it in rutting Time.

See a Print done from the Drawing I made of this Beast,
TAB. III. Fig. 9.

XVIII. *An Account of certain perfect minute Crystal Stones, by J. Parsons, M. D. & F. R. S.*

Read June 27. 1745. THE Drawing here annexed, TAB. III.

Fig. 10. represents a small Crystal magnified; it is one of a great Number brought by a very curious Gentleman from *Gibraltar*, who has caused many of them to be set in Buckles of different Kinds, for the Wear of his Lady and himself: And altho' they are formed and polish'd by Nature; yet they look very bright, and produce a very good Effect in the Buckles.

They were found accidentally. This Gentleman saw a Man cleaving a Rock near that Town, and observ'd a great Quantity of fine black Powder fall from its Crevices; and, being very curious, he examined the Powder, and found these little Stones in Clusters, consisting of no more than twelve or fourteen each; and each Cluster lying at considerable Distances from one another. They are all of the same Form, some less perfect than others, and are in general Hexagonals.

XIX. *A Letter from Mr. John Ellicot, F. R. S. to the President, concerning the specific Gravity of Diamonds.*

Read July 4. 1745.

AS, from some Experiments I have lately had the Opportunity of making, it appears highly probable, that what has formerly

merly been published concerning the specific Gravity of Diamonds, is not to be depended upon; I hope a short Account of these Experiments will not be unacceptable to you, especially as I do not find the least Notice taken of the specific Gravity of Diamonds in any of the Tables published in the *Philosophical Transactions*.

In the Account the Honourable Mr. *Boyle* has given of Diamonds (as published by Dr. *Shaw*, in his Abridgment of that Gentleman's Philosophical Works *), he relates it "as the Opinion of a famous
" and experienced Cutter of Diamonds, that some
" rough Diamonds were considerably heavier than
" others of the same Bigness, especially if they were
" cloudy or foul; and Mr. *Boyle* mentions one that
" weigh'd 8 Grains and $\frac{8}{16}$; which, being carefully
" weigh'd in Water, according to the Rules of
" Hydrostatics, proved to an equal Bulk of that
" Liquor, as $2 \frac{2}{3}$ to 1; so that, as far as could
" be judged by that Experiment, a Diamond weighs
" not thrice so much as Water." And yet, in this Table of specific Gravities, that of a Diamond is said to be to Water as 3400 to 1000, or as 3, 4. to 1; and therefore, according to these two Accounts, there should be some Diamonds, whose specific Gravity shall differ nearly the $\frac{1}{8}$ from others; which I am persuaded, is a much greater Difference than could be expected in any Bodies of the same kind, or that which, on a more nice Examination, will be found to be in Diamonds.

The

* Pag. 83. Vol. V. new Edition of Mr. *Boyle's Works* in folio.

The first Diamonds I had the Opportunity of seeing weigh'd, were two very large ones from the *Brasils*, which were furnished by Mr. *Chace*, a Merchant in *Austin-friers*: The specific Gravities of these were found to be much greater than the heaviest of Mr. *Boyle's*, the one being to an equal Bulk of Water as 3518, and the other as 3521 to 1000, and the Difference between them less than the one-thousandth Part. There were two smaller *Brasil* Diamonds weigh'd at the same time, which indeed were not quite so heavy as the former, the lightest being but as 3501, the other as 3511; but, as these were of the same kind, and comparatively small, I judg'd this Difference could not be much depended on. Having therefore an Opportunity sometime since of a large Parcel of *East-India* Diamonds, I chose out 10, which, both in Shape and Colour, and every other respect, were as different from each other as possible. These being weigh'd in the same Scales and Water as the former, the lightest proved to be as 3512, and the heaviest as 3525; the very near Agreement of these last with each other, and with the former, tho' weigh'd at about eight Months Distance, makes it highly probable, that so great a Difference as appears from the Place above-cited, and Mr. *Boyle's* Table, is not to be found in any Diamonds whatsoever, much less so great a Difference as appears between the lightest of his and the heaviest of mine, being above $\frac{1}{7}$ of the Whole.

I had never made any Experiments myself, by which I could form a Judgment, how much of the Difference between these and former Trials might arise from the different Tempers and Qualities of the

Waters used ; warm Water being lighter than cold, and Pump-Water generally heavier than River-Water. But, taking it for granted, that all Persons who make such Experiments use common and not Mineral Waters, and Waters of the natural Temper, and not heated designedly, I am assured by a Friend, who has made many careful Trials for this particular Purpose (an Account of which he has promised me shall be laid before the *Royal Society*), that the specific Gravity of any Body will not differ above $\frac{1}{2000}$ at the most, on account of the Quality of the Water and Temper taken together ; whereas the heaviest of Mr. *Boyle's* Diamonds, as in his Tables, differs from the lightest of mine by above one Thirty-fifth Part, which is about six times as much as $\frac{1}{2000}$: And yet I can think of no other Way of accounting for the rest of this Difference ; unless it should arise from the Smallness of the Diamonds, or any Defect in the Instruments with which his Experiments were made.

The Scales in which these Diamonds were weigh'd turned very sensibly with the two-hundredth Part of a Grain ; and as one of the Diamonds weighed above 92 Grains, it was capable of being weighed to less than the 18000th Part : several of them were weigh'd twice over both in Water and Air, and the Weights found to agree to the greatest Exactness ; and if to this is added the very near Agreement of the Weights of the several Diamonds, tho' weigh'd at different Times, and at a considerable Distance, from each other, I think it highly improbable, that there could be any considerable Mistake in these Trials ; and therefore

their specific Gravities, as in the following Table, may fully be depended on.

I have set down the Weights of the several Diamonds both in Air and Water, that if any Mistake should have happened, it may be the more easily rectified. I am, Sir, with the greatest Respect,

Your obedient humble Servant,

John Ellicott.

N ^o .	Water	In Air	In Water	Specific Gravity
		Grains	Grains	
	Water			1000
1	A <i>Brazil</i> Diamond, fine Water, rough Coat	92,425	66,16	3518
2	A <i>Brazil</i> Diamond, fine Water, rough Coat	88,21	63,16	3521
3	Ditto. fine bright Coat,	10,025	7,170	3511
4	Ditto. fine bright Coat,	9,560	6,830	3501
5	An <i>East India</i> Diamond, pale blue,	26,485	18,945	3512
6	Ditto bright yellow	23,33	16,71	3524
7	Ditto. very fine Water, bright Coat,	20,66	14,8	3525
8	Ditto. very bad Water, honeycomb Coat,	20,38	14,59	3519
9	Ditto. very hard blewish Cast,	22,5	16,1	3515
10	Ditto. very soft, good Water,	22,615	16,2	3525
11	Ditto. a large red foul in it.	25,48	18,23	3514
12	Ditto. soft bad Water	29,525	21,140	3521
13	Ditto. soft brown Coat,	26,535	18,99	3516
14	Ditto. very deep green Coat,	25,25	18,08	3521

The mean Specific Gravity of the *Brazil* Diamonds appears to be 3513.
 The mean of the *East-India* Diamonds, " " " 3519.
 The mean of Both to be " " " 3517.

XX. *A Letter to Martin Folkes, Esq; President of the Royal Society, from Cromwell Mortimer, M. D. Secr. of the same, concerning the natural Heat of Animals.*

S I R,

June 20. 1745.

Read July 4 1745.

SINCE the complete and full Demonstration of the Circulation of the Blood in Animals by our illustrious Countryman the great Dr. *Harvey*, the Generality of medical Writers have attributed the natural Heat of Animals to the Motion of the Blood in the Blood-vessels, or rather to an Attrition of all the Fluids in the Animal arising from it; which Fluids, from the later Discoveries by Injections and Microscopes, are found to move in conical Canals communicating one with another near the *Apices*, or where the Arteries are the narrowest, soon afterwards growing wider and wider, when the same continued Canals obtain the Name of Veins, and convey back the Fluids they contain to the Heart. They ascribe Heat in an Animal to strong and frequent Contractions of the Heart and Arteries; which Heat* will be the greater, the more dense the Humours are, the more strongly they are propell'd, and the greater the Resistances are, near the Ends of the Arteries. From this Supposition they conclude, that the Heat arises from Attrition; that, by a violent Agitation of the Particles of the Blood and Humours against one another, and especially by the Attrition of them against the Sides of the containing Blood-vessels,

* *Boerhaave Instit. §. 968.*

vessels, there must be great Friction excited, and from that Friction Heat generated; as is easily done by rubbing two Pieces of Wood together, or a Piece of Wood and a Piece of Metal, or two Pieces of Metal, or hard Stones: but it is known, by daily Experience, that either any watery Fluid, or oily or greasy Substance, applied to these Bodies while rubbed, will prevent the Excitation of Heat; as for Instance, the Use of Water in polishing of Glass or Marble, and the greasing or oiling all manner of Wheel-Machines, many of which, for want of that Application, have heated, taken Fire, and been even consumed in Flames of their own exciting. I know of no Experiment, whereby it appears, that any the least Degree of Heat has been generated by the simple or mere mechanical Agitation or Friction of the Particles of any Fluid, either by itself, or mixed with various Fluids; Water, Wine, vinous Spirits, Oils, Quick-silver, either agitated singly or mixed, will by no Force, or Velocity of Motion I ever heard of, produce Heat; nor can the Blood of Animals, when once let out of the Body, be kept either fluid or warm by any the most violent Agitation. Indeed Heat is generated in Fluids in some particular Circumstances, as in those two so commonly known Cases of *Fermentation* and *Effervescence*; which, as they are frequently confounded by Persons not thoroughly versed in chemical Matters, I shall beg Leave to explain the Difference. *Fermentation* is that spontaneous intestine Motion, which, in the Degree of Heat of the universal Temperature of subterraneous Caverns, will, in a few Hours, bring on such a Change in vegetable Juices, or in Water charged with

with a strong Tincture of vegetable Particles (for Fermentation is confined to the Vegetable Kingdom solely) as from a vapid Must or Wort quenching Fire, to make it become more or less inflammable and nourishing of Fire, as it is impregnated with more or fewer of the vegetable Particles, and in the Alembic to afford that volatile subtil inflammable Liquor commonly called vinous Spirits. The Heat produced by Fermentation never exceeds that of the human Body. *Effervescence* arises from an intestine Motion, to be excited in various Sorts of Fluids, either by the Mixture of Fluids with Fluids of different Natures together, or by dropping in Salts or Powders of different Natures into different Fluids: The two most common Opposites, *Acids* and *Alcali's*, on being mixt, cause a great Ebullition or Frothing, but no great Heat; but the Solutions of some Metals in *Aqua fortis* cause intense Heat, and emit Flame: The mixing aromatic Oils with acid mineral Spirits actually kindle, and burn with violent Explosions; and some vegetable Substances, putrefying with Moisture, will sometimes heat so, as to kindle what lies dry above that Part of the Heap where the Putrefaction happens. Thus Dung-Heaps will heat, and Haycocks often kindle into actual Fire.

In these Cases of Effervescence, as there is no adventitious Heat or Fire applied, there must be the Elements of Fire lying hid or dormant in one or other of the Bodies; and it is sufficiently known, by Experiment, that there is Abundance of Air lies dormant in all Bodies both solid and fluid; and it is likewise known, that Fire cannot exert itself without the elastic Assistance of common Air; for Wood
will

will not burn, nor even Gunpowder fire, in the artificial *Vacuum*. It being therefore granted, that there are the Elements of Fire and of Air lying dormant in all Bodies; there is only required such an Action as may set at Liberty the Particles of Air, and the Particles of Fire; by which Action the Particles of Air will recover their Elasticity, and, putting the Particles of Fire in Motion, cause Heat or Warmth, but not Incension or Inflammation; unless the Fire thus agitated meets with a proper *Pabulum*, which *Pabulum* is *Sulphur* only, tho' differently modified, whether under the Appearance of Brimstone, *Bitumen*, Oil, vinous Spirits, vegetable Substances when deprived of their Water, metalline Sulphurs, or the most inflammable of all, animal Sulphur, commonly called by our modern Chemists *Phosphorus*.

Thus in Fermentation, the Fire and Air being let loose, produce a Warmth, but do not kindle, because of the Water predominating; whereas in the Effervescence produced by the Solution of Metals, the Fire meets with the metalline Sulphur, which it kindles, and sometimes causes Explosions; the aromatic Oils containing but little Water, being almost entirely composed of the sulphureous Parts of the Vegetables, immediately kindle, and break out into Flame; and *Phosphorus*, which is nothing but the animal Sulphur, as appears by the curious Account of it given us by that late ingenious Chemist Mr. *Godfrey*, a worthy Member of this *Society*, (see Trans. N^o. 428. p. 69. 70. is so greatly disposed to take Fire, that if it be only exposed a few Minutes to the open Air, it kindles and flames.

Now all Animals, on which Experiments have been made, are found to contain more or less of the phosphoreal Principles; some Insects constantly shine, or emit Light, in the open Air; many Sorts of Fish are luminous, if exposed to the Air a short time; nay even the Bubbles of the Sea-water appear like Fire in the Dark: some Quadrupeds have been observed to emit Light on very slight Friction of their Hair, as the Necks of Horses, the Backs of Cats, and the like; and there are many Instances in our own Species, of many Parts of the Body appearing luminous, and even of the Exhalations from it adhering to the Cloaths, causing them to shine likewise; of which several curious Observations* have lately been laid before this *Society*: These, I think, are convincing Proofs of *Phosphorus* existing, at least in a dormant State, in animal Fluids; and as it is likewise certain, that they all contain Air, it is only necessary to bring the phosphoreal and æreal Particles to Contact, and Heat must of consequence be generated; and was it not for the Superabundance of aqueous Humours in Animals, I do not doubt, but fatal Incensions would frequently happen. This, I think, explains evidently the Cause of animal Heat: Indeed the Heart and Arteries are the Instruments which excite this Heat; but that is not done by the Friction caused by the Circulation of the Humours, but only by the intestine Motion, which the Circulation gives to the several Particles which constitute the Mass of animal Fluids; and as the Velocity of these Fluids is increased, so must the different Particles, of which they consist, come oftener

* See above in this *Transf.* p. 444. 445. 456. 457. &c.
P p p

oftener into Contact; and, consequently, the oftener the phosphoreal and the aëreal meet, the more frequently and greater must the *Nifus's* be to create Heat.

Hippocrates (*Aph.* I. 1.+) mentions the *Θεμνον ἔμφωλον*, *Calidum innatum*. *Galen* takes it for the *Soul*, and more modern Writers have supposed it to be the very *Spirit*, the *Archeus*, and others the *Vital Heat*; but have all treated of it as a certain Degree of Fire existing in Animals; not having any Notion, that the Element of Fire might be absorbed, or lie latent, in fluid Bodies, ready to become active as soon as it meets with Air, or even to kindle, if it meets with sulphureous Particles under proper Circumstances. This I fancy the Antients, in the very earliest Ages of the World, had some Notions of; when they thought proper to communicate to the Vulgar some Shadows only of more profound and real Knowledge under Types and Fables, as handed down to us in the Fictions of the Poets: Of this kind, and quite to our Purpose, I take the Fiction of *Prometheus* stealing Fire from Heaven to animate his Men with, to be one. And, I think, upon this Principle of *Phosphorus* existing in Animals, one may easily explain the Cause of those melancholy Accidents which have happened to some of the human Species, as that of the Lady at *Cesena* in *Italy*, (a) the Carpenter in *Hampshire*, (b) and the Woman lately at *Ipswich* (c); who, it is most probable, were all set on Fire by Lightning: It may be said, Many are struck by Lightning, but not set on Fire; but it is to be remarked, that the Lady at *Cesena* had charged all her Pores and absorbent Vessels with a great Quantity

tity of Camphire ; the Woman at *Ipswich* had drank Plenty of Gin ; and as for the Carpenter, that Circumstance is not recorded of him, whether he was an hard Drinker or not ; which Circumstances must greatly promote the kindling the phosphoreal Fire in them ; and, as this *Pabulum* was conveyed into the most minute capillary Vessels, might produce an almost instantaneous Deflagration and Dissolution of all the solid containing Parts.

Animals appearing more susceptible of electric Fire than other Bodies, greatly confirms my Conjectures of the phosphoreal Principles ; and I should think, that being render'd electric to any high Degree might prove a dangerous Experiment to a Person habituated to a plentiful Use of spirituous Liquors, or to Embrocations with camphorated Spirit of Wine ; ~~on the contrary, in some languid, cold, or worn-out Constitutions,~~ possibly, future Experiments may evince, that Electricity may be used medically, in order to renew and regenerate a proper Quantity of vital Fire, such as is necessary for the conveniently carrying on, and performing the animal Functions.

I hope, Sir, your Goodness will excuse the Haste with which I have penned down these Thoughts, being the Subject of a Letter I wrote to my much respected Preceptor the famous *Boerhaave* *, while I was at *Leyden* above 20 Years ago ; but which not finding

* Who honoured me with an intimate literary Correspondence even to within a few Days of his Death ; it being to me, *Amico Londinensi*, a Friend at *London*, that he wrote that Letter stating his own Case, as published by *Prof. Schnitzus* in his Oration on *Boerhaave's* Death, p. 69, but why that Gentleman suppressed my Name I know not.

a Copy of, and he only telling me, it was a pretty *Hypothesis*, I never reflected upon since, till these electrical Experiments lately read before the *Society*, and those Accounts of luminous Emanations from human Bodies, had brought them back to my Mind; and I think I have now set them a little beyond a mere *Hypothesis*. I am,

S I R,

Your most obliged,

Devoted, humble Servant,

Cromwell Mortimer.

July 4. 1745. the SOCIETY adjourned to Oct. 24.

Printed for C. DAVIS, over-against *Gray's-Inn Gate*
in *Holbourn*, PRINTER to the ROYAL SOCIETY,
M.DCC.XLV.

N. B. A Letter from Dr. *Miles* to Mr. *Baker* having been published in the *Phil. Transf.* N°. 469. as that Gentleman has nothing in View but the Discovery of Truth, he thinks proper to rectify a Mistake he then made, in supposing certain Bodies to have been animal Substances, from their seeming to have a spontaneous Motion in Water: it having since appeared to him that they were only the Seeds of the *Bidens foliis tripartitè divisis. Cæsalp.* 488. TOURNEFORT. p. 462. Tab. 262. that had fallen into the Water, and were possibly possess'd by some Insects which might give them that Motion.

E R R A T A.

In *Transaction* 475. pag. 285. l. 24. for *Calender*, read *Calendar*:
ib. p. 286. l. 1. for *D, G, N* and *O*, read *G, D, N*, and *O*.

In Page 373. Line 23. of this *Transaction* for *Snonebergam* read
Sonnebergam: B. p. 393. l. 1. for *Mr.* read *Dr. Bamber*: *Ibid.*
p. 457. l. 19. for *Act. Med. Phil. & Hafn.* read *Acta Med. & Philos.*
Flammarion.

PHILOSOPHICAL TRANSACTIONS.

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November, and December, 1745.

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Printed for C. DAVIS. over-against *Gray's-Inn Gate*
in *Holbourn*, PRINTER to the ROYAL SOCIETY,
M.DCC.XLV.

I. *Experiments and Observations, tending to illustrate the Nature and Properties of Electricity: By William Watſon, Apothecary, F. R. S.*

A Letter to Martin Folkes, Eſq; Pr. R. S.

S I R,

Read at ſeveral Meetings of the Royal Society, between March 28, and October 24, 1745. here printed with Alterations.

I. **T**HE SOCIETY having heard, from ſome of their Correſpondents in * *Germany*, that what they call a Vegetable Quinteſſence had been fired by Electricity, I take this Opportunity to acquaint you, that, on *Friday* Evening laſt, I ſucceeded, after having been diſappointed in many Attempts, in ſetting Spirits of Wine on fire by that Power.

The preceding Part of the Week had been remarkably warm, and the Air very dry; than which nothing is more neceſſary towards the Succeſs of electrical Trials: To theſe I may add, that the Wind was then Eaſterly, and inclining to freeze. I that Evening uſed a glaſs Sphere, as well as a Tube; but I always find myſelf capable of ſending forth much more Fire from the Tube than from the Sphere, probably, from not being ſufficiently uſed to the laſt.

Q q q

I had

* See an Account of Prof. *Winckler's* Book of *Electricity* in theſe *Trans.* N^o. 474. p. 166. Prof. *Hollman, Trans.* N^o. 475. p. 239. Dr. *Miles, ib.* p. 290. *Winckler's Experiments, ib.* p. 307. Mr *de Bozes, Trans.* N^o. 475. p. 419. C. M.

I had before observ'd, that, altho' * Non-electric Bodies, made electrical, lose almost all that Electricity, by coming either within or near the Contact of *Non-electrics* not made electrical. It happens otherwise with regard to *Electrics per se*, when excited by rubbing, patting, &c. ; because from the rubbed Tube I can sometimes procure five or six Flashes from different Parts ; as though the Tube of two Feet long, instead of being one continued Cylinder, consisted of five or six separate Segments of Cylinders, each of which gave out its Electricity at a different Explosion.

The Knowledge of this Theorem is of the utmost Consequence towards the Success of electrical Experiments ; inasmuch as you must endeavour, by all possible means, to collect the Whole of this Fire at the same time. Professor *Hollman* seems to have endeavour'd at this, and succeeded, by having a tin Tube ; in one End of which he put a great many Threads, whose Extremities touch'd the Sphere when in Motion, and each Thread collected a Quantity of electrical Fire, the Whole of which center'd in the tin Tube, and went off at the other Extremity. Another thing to be observed is, to endeavour to make the Flashes follow each other so fast, as that a
second

* I call *Electrics per se*, or originally, *Electrics*, those Bodies, in which an attractive Power towards light Substances is easily excited by Friction ; such as Glass, Amber, Sulphur, Sealing-wax, and most dry Parts of Animals, as Silk, Hair, and feathers. I call *Non-electrics*, or Conductors of Electricity, those Bodies, in which the above Property is not at all, or very slightly perceptible, such as Wood, Animals living or dead, Metals, and vegetable Substances. See *Gray*, *De Fly*, *Deaguliere*, *Wheler*, in the *Philosophical Transactions*, N^o. 417. 422. 428. 431. 436. 439. 444. 453. 454. 460.

second may be visible before the first is extinguish'd. When you transmit the electrical Fire along a Sword, or other Instrument, whose Point is sharp, it often appears as a Number of disseminated Sparks, like wet Gunpowder or Wild-fire: But if the Instrument has no Point, you generally perceive a pure bright Flame, like what is vulgarly call'd the *Blue ball*, which gives the Appearance of Stars to fired Rockets.

The following is the Method I made use of, and was happy enough to succeed in. I suspended a Poker in silk Lines; at the Handle of which I hung several little Bundles of white Thread, the Extremities of which were about a Foot at right Angles from the Poker. Among these Threads, which were all attracted by the rubbed Tube, I excited the greatest electrical Fire I was capable, whilst an Assistant, near the End of the Poker, held in his Hand a Spoon, in which were the warm Spirits. Thus the Thread communicated the Electricity to the Poker, and the Spirit was fired at the other End. It must be observ'd in this Experiment, that the Spoon with the Spirit must not touch the Poker; if it does, the Electricity, without any Flashing, is communicated to the Spoon, and to the Assistant in whose Hand it is held, and so is lost in the Floor.

By these means I fired several times not only the æthereal Liquor or *Phlogiston* of *Frobenius*, and rectified Spirit of Wine, but even common proof Spirit. These Experiments, as I before observed, were made last *Friday* Night, the Air being perfectly dry. *Sunday* proved wet, and *Monday* somewhat warm; so that the Air was full of Vapour, Wind South-west, and

cloudy. Under these Disadvantages, on *Monday* Night I attempted again my Experiments ; they succeeded, but with infinitely more Labour than the preceding, because of the Unfitness of the Evening for such Trials. Your Candour will not permit you to think my Minuteness trivial, with regard to the Circumstances of the Weather, who know how many Things must concur to make these Experiments succeed. I shall wait with Impatience for a proper Opportunity to have these Experiments repeated in your Presence ; and am, with the utmost Respect,

S I R,

Aldersgate-Street, March 27.

1745.

Your most obedient

Humble Servant,

W. Watson,

II.

A Letter to the ROYAL SOCIETY.

Gentlemen,

Read April 25.
1745.

I Lately acquainted you, that I had been able to fire Spirit of Wine, *Phlogiston* of *Frobenius*, and common proof Spirit, by the Power of Electricity. Since which (till Yesterday) we have had but one very dry fine Day; *viz. Monday, April 15.* Wind E. N. E. ; when, about Four o'Clock in the Afternoon, I got my *Apparatus* ready, and fired the Spirit of Wine four times from the Poker as before, three times from the Finger of a Person electrified, standing upon a Cake of Wax, and

and once from the Finger of a second Person standing upon Wax, communicating with the first by means of a Walking-cane held between their Arms extended. The horizontal Distance in this Case between the glass Tube and the Spirit was at least ten Feet.

You all know, that there is the repulsive Power of Electricity, as well as the attractive; inasmuch as you are able, when a Feather, or such-like light Substance, is replete with Electricity, to drive it about a Room, which Way you please. This repulsive Power continues, until either the Tube loses its excited Force, or the Feather attracts the Moisture from the Air, or comes near to some non-electric Substance; if so, the Feather is attracted by, and its Electricity lost in, whatever Non-electric it comes near. In electrified Bodies, you see a perpetual Endeavour to get rid of their Electricity. This induced me to make the following Experiment.

I placed a Man upon a Cake of Wax, who held in one of his Hands a Spoon with the warm Spirits, and in the other a Poker with the Thread. I rubbed the Tube amongst the Thread, and electrified him as before. I then ordered a Person not electrified to bring his Finger near the Middle of the Spoon; upon which, the Flash from the Spoon and Spirit was violent enough to fire the Spirit. This Experiment I then repeated three times.

In this Method, the Person by whose Finger the Spirit of Wine is fired, feels the Stroke much more violent, than when the electrical Fire goes from him to the Spoon. This Way, for the sake of Distinction, we will call the repulsive Power of Electricity.

The

The late Dr. *Desaguliers* has observed, in his excellent Dissertation concerning Electricity, ‘ That there is a sort of Capriciousness attending these Experiments, or something unaccountable in their *Phænomena*, not to be reduced to any Rule. For sometimes an Experiment, which has been made several times successively, will all at once fail.’ Now I imagine, that the greatest Part, if not the Whole of this Matter, depends upon the Moisture or Dryness of the Air; a sudden though slight Alteration in which, perhaps not sufficient to be obvious to our Faculties, may be perceived by the very subtle Fire of Electricity. For,

1st; I conceive, that the Air itself (as has been observed by Dr. *Desaguliers*) is an Electric *per se*, and of the vitreous Kind; therefore it repels the Electricity arising from the glass Tube, and disposes it to electrify whatever non-electrical Bodies receive the *Effluvia* from the Tube.

2dly, That Water is a Non-electric, and, of consequence, a Conductor of Electricity. This is exemplified by a Jet of Water being attracted by the Tube, from either Electrics *per se* conducting Electricity, and Non-electrics more readily when wetted; but what is more to my present Purpose, is, that if you only blow through a dry glass Tube, the Moisture from your Breath will cause that Tube to be a Conductor of Electricity.

Those being premised, in proportion as the Air is replete with watery Vapours, the Electricity arising from the Tube, instead of being conducted, as proposed, is, by means of these Vapours, communicated

to the circumambient Atmosphere, and dissipated as fast as excited.

This Theory has been confirmed to me by divers Experiments, but by none more remarkably than on the Evening of the Day I made those before-mention'd; when the Vapours, which in the Afternoon, by the Sun's Heat, and a brisk Gale, were dissipated, and the Air perfectly dry, descended again in great Plenty, upon the Absence of both, and in the Evening was very damp. For between seven and eight o' Clock, I attempted again the same Experiments in the same manner, without being able to make any of them succeed; though all those mentioned in this Paper, with others of less Note, were made in less than half an Hour's time.

I am the more particular in this, being willing to save the Labour of those, who are desirous of making this Kind of Trials. For, although some of the lesser Experiments may succeed almost at any time, yet I never could find, that the more remarkable ones would succeed but in dry Weather. I am,

Gentlemen,

Your most obedient,

*London, April 25.
1745.*

Humble Servant,

W. Watfon.

III.

*A Letter to the ROYAL SOCIETY.**Gentlemen,*

Read Octob. 24. 1745. **I**N some Papers I lately did myself the Honour to lay before you, I acquainted you of some Experiments in Electricity; particularly I took notice of having been able to fire Spirit of Wine by what I called the repulsive Power thereof; which I have not heard had been thought of by any of those *German* Gentlemen, to whom the World is obliged for many surprising Discoveries in this Part of Natural Philosophy.

How far, strictly speaking, the Spirit, in this Operation, may be said to be fired by the repulsive Power of Electricity, or how far that Power, which repels light Substances when fully impregnated with Electricity, fires the Spirit, may probably be the Subject of a future Inquiry; but, as I am unwilling to introduce more Terms into any Demonstration than what are absolutely necessary for the more ready Conception thereof, and as inflammable Substances may be fired by Electricity two different Ways, let the following Definitions at present suffice of each of these Methods.

But first giye me Leave to premise, that no inflammable Substances will take Fire, when brought into or near the Contact of Electrics *per se* excited to Electricity. This Effect must be produced by non-electrical Substances impregnated with Electricity received from the exciting Electrics *per se*. But to return:

1st, I suppose that inflammable Substances are fired by the attractive Power of Electricity, when this Effect arises from their being brought near excited Non-electrics.

2^{dly}, That inflammable Substances are fired by the repulsive Power of Electricity; when it happens, that the inflammable Substances, being first electrified themselves, are fired by being brought near Non-electrics not excited.

This Matter will be better illustrated by an Example. Suppose that either a Man standing upon a Cake of Wax, or a Sword suspended in silk Lines, are electrified, and the Spirit, being brought near them, is fired, this is said to be performed by the attractive Power of Electricity. But if the Man electrified, as before, holds a Spoon in his Hand containing the Spirit, or the same Spoon and Spirit are placed upon the Sword, and a Person not electrified applies his Finger near the Spoon, and the Spirit is fired from the Flame arising from the Spoon and Spirit upon such Application, this I call being fired by the repulsive Power. Of the two mention'd Kinds I generally find the repulsive Power strongest.

Since my last Communication, the Spirit has been fired both by the attractive and repulsive Power thro' four Persons standing upon electrical Cakes, each communicating with the other, either by the Means of a Walking-cane, a Sword, or any other non-electric Substance. It has likewise been fired from the Handle of a Sword held in the Hand of a third Person.

I have not only fired *Frobenius's Phlogiston*, rectified Spirit, and common proof Spirit, but also *Sal*

volatile oleosum, Spirit of Lavender, dulcified Spirit of Nitre, Peony-water, *Daffy's Elixir*, *Helvetius's* Stryptic, and some other Mixtures where the Spirit has been very considerably diluted; likewise distilled vegetable Oils, such as that of Turpentine, Lemon, Orange-peels, and Juniper; and even those of them which are specifically heavier than Water, as Oil of Sassafras; also resinous Substances, such as Balsam *Cappivi*, and Turpentine; all which send forth, when warmed, an inflammable Vapour. But expressed vegetable Oils, as those of Olives, Linseed, and Almonds, as well as Tallow, all whose Vapours are un-inflammable, I have not been able yet to fire; but these indeed will not fire on the Application of lighted Paper. Besides, if these last would fire with lighted Paper, unless their Vapours were inflammable, I can scarce conceive they would fire by Electricity; because, in firing Spirits, &c. I always perceive, that the Electricity snaps, before it comes in Contact with their Surfaces, and therefore only fires their inflammable Vapours.

As an excited Non electric emits almost all its Fire, if once touch'd by a Non electric not excited, I was desirous of being satisfy'd, whether or no the Fire emitted would not be greater or less in proportion to the Volume of the electrified Body. In order to this, I procured an Iron Bar about 5 Feet long, and near 170 Pounds in Weight; this I electrified lying on ~~Cakes~~ of Wax and Resin, but observed the Flashes arising therefrom not more violent than those from a common Poker. In making this Experiment, being willing to try the repulsive Force, it once happen'd, that whilst the Bar was at one End electrifying,

a Spoon

a Spoon lay upon the other ; and, upon an Assistant's pouring some warm Spirit into the Spoon, the electrical Flash from the Spoon snapped, and fired the first Drop of the Spirit ; which unexpectedly fired not only the whole Jett as it was pouring, but kindled likewise the whole Quantity in the Por, in which I usually have it warm'd.

I find, in firing inflammable Substances from the Finger of a Man standing upon Wax, that, *cæteris paribus*, the Success is more constant, if the Man, instead of holding the Thread (the Use of which I communicated in a former Paper) in his Hand, the Thread is suspended at the End of an iron Rod held in one Hand, and he touches the Spirit with one of the Fingers of the other.

If a Man, standing upon the electrical Cake with a Dish or deep Plate of Water in one Hand, and the iron Rod with the Thread in the other, is made electrical, and a Person not electrified touches any Part either of the Plate or Water, the Flashes of Fire come out plentifully ; and where-ever you bring your Finger very near, the Water rises up in a little Cone, from the Point of which the Fire is produced, and your Finger, though not in actual Contact, is made wet. The same Experiment succeeds through three or more People.

In firing inflammable Substances, the Person who holds the Spoon in his Hand to receive the electrical Flashes, when the Finger of the electrified Person is brought near thereto, not only feels a Tingling in his Hand, but even a slight Pain up to his Elbow. This is most perceptible in dry Weather, when the Electricity is very powerful.

There is considerable Difficulty in firing Electrics *per se*, such as Turpentine and Balsam *Capivi*, by the repulsive Power of Electricity; because, in this Case, these Substances will not permit the Electricity to pass through them: Therefore, when you would have this Experiment succeed, the Finger of the Person who is to fire them, is to be applied as near to the Edge as possible of these Substances when warmed in a Spoon, that the Flashes from the Spoon (for these Substances will emit none) may snap, where they are spread the thinnest, and then fire their *Effluvia*. This Experiment, as well as several others, serves to confute that Opinion, which has prevailed with many, that the Electricity floats only upon the Surfaces of Bodies.

If an electrical Cake is dipp'd in Water, it is thereby made a Conductor of Electricity; the Water hanging about it transmitting the electrical *Effluvia* in such a manner, that a Person standing thereon can by no means be electrified enough to attract the Leaf Gold at the smallest Distance; though the Person standing upon the same Cake when dry, attracted a Piece of fine Thread hanging at the Distance of two Feet from his Finger. We must here observe, that the Cake being of an unctuous Substance, the Water will no-where lie uniformly thereon, but adhere in separate *Molecules*; so that, in this Instance, the Electricity jumps from one Particle of Water to another; till the Whole is dissipated.

From the Appearance of the Threads, amongst which I rub the Tube, I can frequently judge, though the Spirit may be many Feet distant from them, whether or no it will fire; because, when the Person

sons standing upon the Wax are made electrical enough to fire the Spirit, the Threads repel each other at their lower Parts, where they are not confin'd, to a considerable Distance; and this Distance is in Proportion as the Threads are made electrical.

If two Persons stand upon electrical Cakes at about a Yard's Distance from each other, one of which Persons, for the sake of Distinction, we will call *A*, the other *B*; if *A*, when electrified, touches *B*, *A* loses almost all his Electricity at that Touch only, which is received by *B*, and stopped by the electrical Cake: If *A* is immediately electrified again to the same Degree as before, and touches *B*, the Snapping is less upon the Touch; and this Snapping, upon electrifying *A*, grows less and less, till *B*, being impregnated with Electricity, though received at Intervals, the Snapping will no longer be sensible.

That Glass will repel and not conduct the Electricity of Glass, has been mention'd by others, who have treated of this Subject; but the Experiments to determine this Matter must be conducted with a great deal of Caution; for, unless the glass Tube, intended to conduct the Electricity, be as warm as the external Air, it will seem to prove the contrary, unless in very dry Places and Seasons. Thus I sometimes have brought a cold though dry glass Tube near three Feet long into a Room where there has been a Number of People; when, upon placing the Tube upon silk Lines, and laying some Leaf-Silver upon a Card at one End, and rubbing another glass Tube at the other, the Silver has, contrary to Expectation, been thrown off as readily as from an iron Rod. At first I was surpris'd at this Appearance;

ance; but then conjectur'd, that it must arise from the Coldness of the Glass, condensing the floating Vapour of the Room. In order then to obviate this, I warm'd the Tube sufficiently, and this Effect was no longer produc'd, but the Silver lay perfectly still.

If a Number of Pieces of finely spun Glass, cut to about an Inch in Length, little Bits of fine Wire of the same Length, of what Metal you please, and small Cork-Balls, are either put all together, or each by themselves, into a dry pewter Plate, or upon a Piece of polished Metal, they make, in the following Manner, a very odd and surprising Appearance. Let a Man, standing upon electrical Cakes, hold this Plate in his Hand, with the Bits of Glass, Wire, &c. detached from each other, as much as conveniently may be; when he is electrified, let him cause a Person standing upon the Ground to bring another Plate, his Hand, or any other Non-electric, exactly over the Plate, containing these Bodies. When his Hand, &c. is about eight Inches over them, let him bring it down gently: As it comes near, in proportion to the Strength of the Electricity, he will observe the Bits of Glass first raise themselves upright; and then, if he brings his Hand nearer, dart directly up, and stick to it without snapping. The Bits of Wire will fly up likewise, and as they come near the Hand snap aloud; you feel a smart Stroke, and see the Fire arising from them to the Hand at every Stroke: Each of these, as soon as they have discharged their Fire, falls down again upon the Plate. The Cork-Balls also fly up, and strike your Hand, but fall again directly. You have a constant Succession of these Appearances, as long as you conti-

nue to electrify the Man in whose Hand the Plate is held; but if you touch any Part either of the Man or Plate, the Pieces of Glass, which before were upon their Ends, immediately fall down.

Some few Years ago, Sir *James Lowther* * brought some Bladders fill'd with inflammable Air, collected from his Coal-mines, to the *Royal Society*. This Air flamed, upon a lighted Candle being brought near it. This Inflammability has occasion'd many terrible Accidents. Mr. *Maud*, a worthy Member of this *Society*, made at that time, by Art, and shew'd the *Society*, Air exactly of the same Quality. I was desirous of knowing if this Air would be kindled by electrical Flashes. I accordingly made such Air, by putting an Ounce of Filings of Iron, an Ounce of Oil of Vitriol, and four Ounces of Water, into a *Florence* Flask; upon which an Ebullition ensued, and the Air, which arose from these Materials, not only fill'd three Bladders, but also, upon the Application of the Finger of an electrified Person, took Flame, and burnt near the Top and out of the Neck of the Flask a considerable Time. When the Flame is almost out, shake the Flask, and the Flame revives. You must, with your Finger dipped in Water, moisten the Mouth of the Flask as fast as it is dried by the Heat within, or the Electricity will not fire it: Because the Flask, being an Electric *per se*, will not snap at the Application of the Finger, without the Glass being first made non-electric by wetting. It has sometimes happen'd, if the Finger has been applied before the inflammable Air has found a ready *Exit* from the Mouth of the Flask, that the Flask has
filled

* See these *Transf.* N°. 442. p. 282.

filled the Flask, and gone off with an Explosion equal to the Firing of a large Pistol; and sometimes indeed it has burst the Flask. The same Effect is produced from Spirit of Sea Salt, as from Oil of Vitriol; but as the Acid of Sea-Salt is much lighter than that of Vitriol, there is no Necessity to add the Water in this Experiment.

Those who are not much acquainted with Chemical Philosophy, may think it very extraordinary, that, from a Mixture of cold Substances, which, both conjunctly and separately, are uninflamable, this very inflammable Vapour should be produced. In order to solve this, it may not be improper to premise, that Iron is compounded of a sulphureous as well as a metallic Part. This Sulphur is so fixed, that, after heating the Iron red hot, and even melting it ever so often, the Sulphur will not be disengaged therefrom: But, upon the Mixture of the vitriolic Acid, and by the Heat and Ebullition which are almost instantly produced, the metallic Part is dissolved, and the Sulphur, which before was intimately connected therewith, being disengaged, becomes volatile. This Heat and Ebullition continue, till the vitriolic Acid is perfectly saturated with the metallic Part of the Iron; and the Vapour, once fired, continues to flame, until, this Saturation being perfected, no more of the Sulphur flies off.

I have heretofore mentioned, how considerably perfectly dry Air conduces to the Success of these Experiments; but we have been lately informed, by an Extract of a Letter, that Abbé *Nolet* was of Opinion, that they would succeed in wet Weather, provided the Tubes were made of Glass tinged blue with Zaffer. I have procured Tubes of this sort, but,
after

after giving them many candid Trials, I cannot think them equal to their Recommendation. I first tried one of them in a smart Shower of Rain after a dry Day, when the Drops were large, and the Spirit fired three times in about four Minutes : The same Effect succeeded, under the same Circumstances, from the white one; but, after three or four Hours raining, when the Air was perfectly wet, I never could make it succeed. And, to illustrate this Matter further, I have been able, when the Weather has been very dry, with once rubbing my Hand down this blue Tube, and applying it to the End of an iron Rod six Feet long, to throw off several Pieces of Leaf-Silver lying upon a Card at the other End of this Rod ; whereas I never have been able to throw it off by any means in very wet Weather. Besides, I am of Opinion, that, after the electrical Fire is gone from the Tube, the Tube has no Share in the conducting of it: My Sentiments on that Head I laid before you in a former Paper: For if the silk Lines are wetted, they diffuse all the Electricity; and the same Effects happen, when the Air is wet, be your Glass of what Colour it will.

It may not be improper here to observe, that Zaffer, which is used by the Glass-makers and Enamellers, is made of Cobalt or Mundick calcined after the subliming the Flowers. This being reduced to, a very fine Powder, and mixt with twice or thrice its own Weight of finely powder'd Flints, is moistened with Water, and put up in Barrels, in which it soon runs into an hard Mass, and is called Zaffer.

A dry Sponge hanging by a Packthread at the End of an electrified Sword, or from the Hand of an

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electrified

electrified Man, gives no Signs of being made electrical. If it is well soak'd in Water, who ever it is touch'd, you both see and feel the electrical Spark. Not only so, but, if it is so full of Water that it will from the Sponge, those Drops in a dark Room, receiv'd upon your Hand, not only flash and snap, but you perceive a pricking Pain. If you hold your Hand, or any non-electrical Substances, very near, the Water, which had ceased dropping, when the Sponge was not electrified, drops again upon its being electrified; and the Drops fall in Proportion to the receiv'd Electricity, as though the Sponge were gently squeez'd between your Fingers. I was desirous to know if I was able to electrify a Drop of cold Water, dropping from the Sponge, enough to fire the Spirit; but, after many unsuccessful Trials, I was forced to desist; because the cold Water dropping from the Sponge not only cool'd the Spirit too much, but also render'd it too weak: Likewise every Drop carried with it great Part of the Electricity from the Sponge.

I then consider'd, in what manner I could give a Tenacity to the Water sufficient to make the Drops hang a considerable Time; and this I brought about by making a Mucilage of the Seeds of Fleawort. A wet Sponge then, squeez'd hard, and fill'd with this cold Mucilage, was held in the Hand of an electrified Man, when the Drops, forced out by the Electricity, assisted by the Tenacity of the Liquor, hung some Inches from the Sponge; and, by a Drop of this, I fired not only the Spirit of Wine, but likewise the inflammable Air before-mentioned, both with and without the Explosion. What an extraordinary

ordinary Effect is this, That a Drop of cold Water (for the Seeds contribute nothing, but add Consistence to the Water) should be the *Medium* of Fire and Flame ?

Camphor is a vegetable Resin, and, of consequence, an Electric *per se*. This Substance, notwithstanding its great Inflammability, will not take Fire from the Finger of a Man, or any other Body electrified, tho' made very warm, and the Vapours arise therefrom in great Abundance ; because, neither Electrics *per se* excited, or electrified Bodies, exert their Force by snapping upon Electrics *per se*, though not excited. If you break Camphor small, and warm it in a Spoon, it is not melted by Heat like other Resins ; but, if that Heat were continued, it would all prove volatile. To Camphor thus warm'd, the Finger of an electrified Man, a Sword, or such-like, will, in snapping, exert its Force upon the Spoon, and the circumambient Vapour of the Camphor will be fired thereby, and light up the whole Quantity exposed. The same Experiment succeeds by the repulsive Power of Electricity.

A Poker, thoroughly ignited, put into Spirit of Wine, or into the distilled Oil of Vegetables, produces no Flame in either. It indeed occasions the Vapours to arise from the Oil in great Abundance ; but if you electrify this heated Poker, the electrical Flashes presently kindle Flame in either. The Experiment is the same with Camphor. These Experiments, as well as the following, sufficiently evince, that the electrical Fire is truly Flame, and that extremely subtil.

I have made several Trials in order to fire Gunpowder alone, which I tried both warm and cold, whole and powder'd, but never could succeed: And this arises, in part, from its Vapours not being inflammable, and in part from its not being capable of being fir'd by Flame; unless the Sulphur in the Composition is nearly in the State of Accension. This we see, by putting Gunpowder into a Spoon with rectified Spirit, which, when lighted, will not fire the Powder, till, by the Heat of the Spoon from the burning Spirit, the Sulphur is almost melted. Likewise, if you hold Gunpowder ground very fine in a Spoon over a lighted Candle, or any other Flame, as soon as the Spoon is hot enough to melt the Sulphur, you see a blue Flame, and instantly the Powder flashes off. The same Effects are observed in the *Pulvis fulminans*, compos'd of Nitre, Sulphur, and fixed alkaline Salt. Besides, when the Gunpowder is very dry, and ground very fine, it (as you please to make the Experiment) is either attracted, or repell'd; so that, in the first Case, the End of your Finger, when electrified, shall be cover'd over with the Powder, though held at some Distance; and in the other, if you electrify the Powder, it will fly off at the Approach of any non-electrified Substance, and sometimes even without it. But I can, at Pleasure, fire Gunpowder, and even discharge a Musket, by the Power of Electricity, when the Gunpowder has been ground with a little Camphor, or with a few Drops of some inflammable chemical Oil. This Oil somewhat moistens the Powder, and prevents its flying away: The Gunpowder then being warm'd in a Spoon, the electrical
Flashes

Flashes fire the inflammable Vapour, which fires the Gun-powder : But the Time between the Vapour firing the Powder is so short, that frequently they appear as the same, and not successive Operations, wherein the Gun-powder itself seems fired by the Electricity : And, indeed, the first time this Experiment succeeded, the Flash was so sudden and unexpected, that the Hand of my Assistant, who touch'd the Spoon with his Finger, was considerably scorched. So that there seems a fourth Ingredient necessary to make Gun-powder readily take Fire by Flame ; and That such a one as will heighten the Inflammability of the Sulphur.

In common Cases, the lighted Match, or the little Portion of red-hot Glass, which falls among the Powder, and is the Result of the Collision from the Flint and Steel, fires the Charcoal and Sulphur, and these the Nitre. But if to these three Ingredients you add a fourth, *viz.* a vegetable chemical Oil, and gently warm this Mixture, the Oil, by the Warmth, mixes intimately with the Sulphur, lowers its Consistence, and makes it readily take Fire by Flame.

In these Operations, notwithstanding I always made use of the finest-scented Oils of Orange peel, Lemons, and such-like, yet, upon the least warming the Mixture, the rank Smell of Balsam (*i. e.* of the ready Solution of Sulphur) was very obvious.

II. *A Proposal to bring small passable Stones soon and with Ease out of the Bladder : By the Reverend Stephen Hales, D. D.*

Read Octob. 31.
1746. here printed
with Alterations

B EING present, *February 4. 1744-5.* when the late Right Honourable the Earl of Orford (after having taken for two Months Dr. *Jurn's Lixivum*) voided at once eleven pretty large nearly cubical Fragments of larger Stones, which were involved in coagulated Blood and Urine; and, a few Hours after, fifteen more at once, in the same manner; in all thirty two that Day; some of which were as large as were possibly passable; it hence immediately occurred to my Thoughts, that all passable Stones which have lately fallen from the Kidneys into the Bladder, or which have broken off from larger ones, might readily and easily be brought out thence, by conveying into the empty Bladder, by a *Catheter*, some very mucilaginous Substance, such as Syrup of Marsh-mallows, or a Solution of Gum Arabic, or Barley-water. Such Substances would bring the Stones away soon, and with great Ease to the Patient; and thereby not only prevent much teasing Pain, by fruitlessly endeavouring to bring them away with the weak Force of thin Urine, but also effectually to secure the Patient from the Danger of their growing too big to come away, by long continuing in the Bladder.

And what strongly evinces the Reasonableness of this Proposal, is, that, on opening the Bladder of his Lordship, there were no Stones found remaining,
except

except two small Grains, which were involved in the Folds in the Neck of the Bladder.

If, on Trial, any Stones shall be found too big to pass off, the Patient is but where he was before ; and if any shall be of such a Size as to enter the *Urethra* but Part of the Way, they may be pushed back, or cut out, according as their Situation shall happen to be.

And further to evince the Reasonableness of this Proposal, I made the following Experiments ; *viz.* In order to shew the comparative Force, with which Fluids of different Degrees of Density and Tenacity will impel Stones, I took a glass Tube, which was an Inch in Diameter within, and fourteen and half Inches deep ; and, having fill'd it full of Urine, I put into it a nearly cubical Piece of a large Stone, taken out of a human Bladder, which weigh'd seven and half Grains ; and, standing by a Clock whose *Pendulum* beat Seconds, I found, by repeated Trials, that the Stone was a Second and a Quarter in descending through the fourteen and half Inches Depth of Urine.

The Experiment being tried with the same Stone in Oil of Olives, it was five and three Quarters Seconds in descending : So that the Resistance of the Oil to the falling Stone was 4.6, that is, more than four times greater than the Resistance of the Urine ; and, consequently, the impelling Force of Oil to propel a Stone in passing thro' a narrow Tube, would be proportionably so much greater than that of Urine, were their Velocities equal. .

When an Ounce of Gum-Arabic was dissolved in half a Pint of Water, the Stone descended in

two

two Seconds; with two Ounces, in three Seconds; with three Ounces, in four Seconds.

In a Decoction of warm Barley-water, which was so thick as to be a tender Jelly when cold, the Stone was forty-five Seconds in descending, that is, thirty-five times slower than in Urine; and, consequently, the impelling Force of Urine is thirty-five times less than that of this Mucilage, in case their Velocities were equal.

This Mucilage was, as I guess, of a due Consistence for the Purpose; for it was about the Thickness of Lord *Orford's* coagulated Blood and Urine. Equal Quantities of Blood and Urine will continue a thick *Coagulum* for many Weeks, without any Separation.

But as the Velocity, with which such mucilaginous Substances pass thro' small Tubes, is considerably less than the Velocity with which Urine will pass; supposing the Forces with which they are impelled to be equal; it was requisite to determine those different Velocities by Experiments: And, in order to it, I put half a Pint of the same blood-warm Decoction of Barley into a glass Vessel, where its Depth was near eight Inches, and therefore its mean Depth near four Inches. It run out at the Bottom in about fifty Seconds thro' a glass Tube, whose Bore was $\frac{7}{16}$ th Inch Diameter; its Length two Inches: And, on repeating the same Experiment twice, as the Decoction grew cooler and cooler, it was about eighty and then ninety Seconds in running out; whereas a like Quantity of Urine ran out thro' the same Tube in eighteen Seconds.

Now, supposing the Velocities, at a *Medium*, thro' the *Urethra*, to be as seventy-two to eighteen, then the Velocity of the Urine will be three Fourths greater

greater than that of the Mucilage of Barley. Taking therefore three Fourths from Thirty-six, the Force of the Mucilage, the Remainder nine is the Force with which the Mucilage will impel the Stone ; and, consequently, the impelling Force of the Mucilage, in the Neck of the Bladder and in the *Urethra*, will be nine times greater than that of Urine ; besides the Advantage of greater Slipperiness which it gives to the *Urethra*.

III. *An Account of some Experiments, lately made in Holland, upon the Fragility of unannealed glass Vessels ; communicated to the President.*

Read Oct. 31. 1745. **T**HE following Paper contains the Account of several Experiments of an odd Nature, that have lately been tried both in *Italy* and in *Holland*, upon some unannealed glass Phials ; that is to say, such as have been exposed to the Air as soon as blown, without passing through the Operation that is commonly called Annealing.

The excessive Fragility of these Sorts of Glasses must have been observed, as long as the Art of making Glass has been in Use: it having been found, that almost all the Vessels that were made of such Glass were entirely useless upon that Account ; as being subject to break and fly, almost constantly, of themselves, and that even frequently before they were well cold.

It was therefore to remedy this Inconveniency that the Practice of Nealing or Annealing them was devised ;

vifed; whereby, paſſing very gradually, in the Space of ſome Hours, through what is called the Leer, from a very intense Degree of Heat to the Temperature of the common Air, they were found to acquire ſuch a Toughneſs or Tenacity, as fitted them for the ſeveral Uſes for which they were reſpectively deſigned.

But ſome of the *Phænomena* depending upon their firſt Brittlenefs, or at leaſt very nearly connected with it, have been often judged to deſerve the Attention of the Curious. One of the firſt very worthy Founders of the *Royal Society*, the Right Honourable Sir *Rob. Moray*, very early gave in his Experiments, which appear in the Register, upon thoſe Drops or *Lachrymæ* of Glaſs, which, inſtead of being nealed, had been immediately quenched in Water, or ſome other Fluid. And the ſame learned Perſon further obſerved, that hollow Balls, made of unnealed Glaſs with a ſmall Hole in them, would ſlie in Pieces with the Heat of the Hand only, if the ſmall Hole, by which the internal and external Air communicated, was but ſtopped with the Finger.

The Glaſſes which the following Paper concerns, have been already mention'd to the *Society* by Mr. *Baker*; who, on the 31ſt Day of *January* laſt, communicated the Extract of a Letter he had then newly received from Dr. *Laurentius Bruni* of *Turin*,* taking notice of the ſame; and relating their remarkable Property of reſiſting very hard Strokes that were given them from without, notwithstanding they at the ſame time ſhivered in Pieces, upon the Shocks they received
by

by the Fall of very light and minute Bodies dropped into their Cavities. And Mr. *Elliot*, having very soon after caused some unnealed Glasses to be made here, repeated with them some of the same Experiments, which he found to answer agreeably to what Dr. *Bruni* had mentioned.

But it will further appear to be remarkable in the present Paper, that, according to the Experiments made abroad upon those Glasses, it is not the Weight alone of the Bodies severally dropped into them, which occasions their Rupture; for some certain Bodies break them with abundantly more Ease than others of the same or even much greater Weights: insomuch that such Phials as are shiver'd to Pieces by the Fall of very small Particles of Flint and some other Substances, are nevertheless capable of resisting the much greater Shock, they receive, in like manner, from a leaden Bullet, tho' some Hundreds of times heavier than the Flint.

The Author of the Paper is Monsieur *Allamand*, a Gentleman of Distinction, Merit, and Learning, in *Holland*, a Person of great Curiosity, and particularly well versed in all the Parts of natural and experimental Knowledge. This Gentleman communicated his Observations to the Hon. *William Bentinck*, Esq; of the *Hague*, a worthy Member of the *Royal Society*; and who was pleased immediately not only to transmit them over to the *President*, but also to oblige him at the same time, with a Number of glass Phials, of the very same Sort as those upon which Monsieur *Allamand's* Experiments had been made; that he might thereby be enabled both to report to the *Society* the Facts

he should take notice of, and to repeat, some of the Experiments themselves in their Presence.

Monsieur *Allamand's* Paper is in *French*; but the Substance of it in *English* is as follows.

Experiments made upon glass Phials, which break with the Stroke of certain Bodies; but which resist the Shocks of others, though much more ponderous.

THESE Glasses have been known some time, and an Account has already been given of them in a Dissertation printed at *Padua* in 1743: The Extract of which Dissertation, published in the *Leipsic Acts* for the Month of *February* last gave me also the Curiosity to repeat in *Holland* the same Experiments that had been already made both in *Italy* and some other Places.

These Glasses only differ from ordinary Phials in this, that they have not been set to cool gradually in what is called the *nealing Furnace*, but have been immediately exposed to the open Air as soon as formed. They may be made of any Shape: I have had some cylindrical with a flat Bottom, others of the Figure of a common drinking Glass, others that were conical, and others again elliptic. The Experiments have equally well succeeded upon all these several Glasses; and all that needs to be observed in the making of them is, to take care that their Bottoms may be thicker than their Sides: And, indeed, the thicker the Bottom is, the easier do the Glasses break. I had one particularly, whose Bot-
tom

tom was above three Fingers Breadth in Thickness, and that flew with as much Ease at least as the thinnest Glass. I have had some others equally thick all over; these have flown also, but with more Difficulty than the others.

These Glasses are capable of resisting very hard Blows coming from without: I have given to some, with a Mallet, Strokes sufficient to drive a Nail into Wood tolerably hard, and they have held good without breaking. They also resist the Shock of several heavy Bodies, that are let fall into their Cavities. Thus I have dropped, from the Height of two or three Feet, Musket-balls, Pieces of Iron, Brass, Tin, Silver, Gold, Antimony, Bismuth, Pyrites, Jasper, and several Sorts of Woods, Ivory, and Bone: All which is indeed no-ways extraordinary; for other Glasses equally thick would also bear the Strokes of the same Bodies; but herein consists what is more surprising.

I took a Shiver of Flint, of the Size of a small Pea; I let it fall into the Glass from the Height of three Inches; and in about two Seconds the Glass flew. And having repeated the same Experiment upon several other Glasses with the same Piece of Flint, the greatest Part broke in the Moment of the Shock, and the others one or two Seconds after it.

I have let fall into different Glasses a Shiver of Flint, of but half the Size of that used in the former Experiment, and the Glasses flew in the same Manner.

Another Bit of Flint, of the Size of a small Lentil, has also produced the same Effect.

Being encouraged with this Success, I let fall into one of my Glasses a Piece of Flint no larger than
a Grain.

Grain of Sand : This was too light to produce any sensible Shock, and accordingly the Glass did not break. In order to try further, I shook the Glass with the small Piece of Stone in it; and nothing following, I repeated the same Experiment upon four other Glasses, none of which broke. I then judged my Experiment to have fail'd, and set by those five Glasses; but, about half an Hour after, one of those Glasses flew, and the other four soon after; insomuch that the Glass which remained the longest entire broke also, about three Quarters of an Hour after its being shook.

Tho' Flint is, of all the Bodies that I have employed, that which has hitherto broken these Glasses with the greatest Ease, it is not however the only Body that produces this *Phænomenon*.

I let fall into one a Sapphire set in a Ring; and tho' the Bottom of the Glass was near an Inch in Thickness, the Sapphire pass'd thro' it as thro' a Spider's Web. The Glass was dispersed on all Sides, and the Ring remain'd upon the Table just where the Glass rested.

A Bit of Porcelane, of the Thickness of half a Line, and the Breadth of two Lines, broke also several Glasses; but that only some Seconds after the Shock.

A Bit of Glass, of the same Size, produced the same Effect; and so did a very small Pebble.

Diamonds of several Sizes have constantly done the same.

A very small Piece of hard-temper'd Steel has broken all these Glasses into which I have dropped

One of those Pellets also that Boys play with, and which they commonly call Marbles, broke a Glass into which it was dropped; but not till four Minutes after its Fall.

Being desirous to know if the Bodies upon which I rested my Glasses contributed any thing to the Ease of their breaking, I repeated the same Experiments, holding the Glasses in my Hand, setting them upright in Clay, placing them on a Down Pillow, and putting them in Water; in all which Cases they broke in the very same Manner. I then half filled one of them with Water, and a Piece of Flint, about the Size of a Pea, broke it.

All the Bodies with which I had yet broken Glasses having been elastic without being ductile, I was willing to inquire, if those Qualities were essentially necessary, tho' I was already satisfied, that all the Bodies that had those Qualities, such as Ivory, for Example, would not produce the Effect. After many Trials, none of which succeeded, I thought of slightly rubbing the Bottoms of some of the Glasses with my Finger, and all those upon which I made that Experiment broke; tho' some of them did not fly till above half an Hour after they had been so rubbed. Thinking, that perhaps the Heat I communicated to them with my Hand might occasion their breaking, to examine whether it was so or not, I poured into several some almost boiling Water, which certainly gave them a much greater Heat than I could have given them with my Hand; but none of those Glasses broke.

I have found in the Animal Kingdom but one sort of Bodies capable of breaking these Glasses,
which

which are Pearls: I dropped one of near a Line Diameter into a Glass, and that Glass broke in about half an Hour.

Tho' the Experiment of rubbing with my Finger had convinced me, that the Stroke or Shock of a falling Body is not always necessary to break these Phials, I thought of scratching with a Flint the Bottom of the Glass, and the Glass immediately broke. To assure myself whether the Scratch I had made was the Occasion of its breaking, I took a Rod of Iron whose End was rounded; I push'd it strongly against the Bottom of the Glass, and the Glass flew. I then did the same, and even push'd much harder, against the Bottoms of several ordinary Glasses, but without any Effect: For tho' these Glasses were much thinner than the others, yet none of them stirr'd.

If the Glasses in Question are every-where extremely thin, they do not break in the Circumstances above-mentioned; I have frequently dropped into such Glasses the same Sorts of Bodies as had broken the thicker ones, but without any Success. I have only met with one that split: And I am not even sure but that the Weight of the Body dropped into it, which was a Stone of some Size, might occasion its breaking.

All the Phials upon which I have yet made these Experiments were of white Glass: I have not had an Opportunity of trying those made of the green.

The Author of the Dissertation, published at *Paris* upon this Subject, pretends to account for all these singular *Phænomena* by saying: That the Bodies dropped into these Phials cause a Concussion that is stronger than the Cohesion of the Parts of the Glass; and that consequently, a Rupture of the same must ensue.

ensue. But why does not a Ball of Gold, Silver, Iron, Copper, or any of the other Bodies which I have tried unsuccessfully, tho' 1000 times heavier, equally cause this Concussion, and break the Glasses? Shall it be said, It is because they are not elastic? Copper, Iron, Silver, and Ivory, are elastic; and as much so as Flint and Porcellane; and surely much more so than the End of one's Finger.

It appears to me, that, before we undertake to give the Solution of these *Phænomena*, we should apply ourselves to the making a much greater Variety of Experiments about them; that we should both try a greater Number of Glasses, and those with a greater Variety of differing Bodies, that we may be able thence to collect at last, in what Classes the several Bodies are to be ranged, that are either fit or unfit for these Purposes: And then it may, perhaps, be Time to inquire, Whether it is from the Principles of Chemistry, or from those of Mechanics, or any other Branch of Natural Philosophy, that we are to seek for the Reasons of the several Facts.

AFTER the reading of this Paper, the *President* produced before the *Society* several of the Phials themselves, which he had received from *Holland*, together with some others he had caused to be made at Mr. *Ceile's* Glass house in *White-Fryars*. He acquainted the Company, that he had yet made himself but few Trials of these Glasses, as he was desirous to preserve them till he could have the Satisfaction of shewing them to the *Society*, and repeating some of Monsieur *Allamand's* chief Experiments in their Presence: That he had designedly broken only four of his

foreign Phials; in all which Matters had succeeded agreeably to the foregoing Relation; and that a fifth, into which he had dropped a leaden Bullet, had flown of itself, about a Quarter of an Hour after; but whether from any Operation of the Bullet, or from the Heat of the Room only, he could not rake upon himself to determine.

After this, he proceeded to make a good Number of Experiments upon the Phials; by all which it appeared,

That none of them, either foreign or *English*, were broken by the Shock of a leaden Bullet weighing 12 Pennyweight: tho' the same was let fall from the Height of above two Feet.

That all the foreign Phials, and several of the *English*, into which a Shiver of Flint, of the Weight of three Grains, was let fall, and that from a Height of only 2 Inches, were broken; most of them instantaneously, and the others within two or three Seconds.

That several of them were broken with a small Shiver of their own broken Glass; but that, generally, this Operation was not so quick, the Phials sometimes not flying till two or three Minutes after the Shock.

That several of them were shiver'd immediately, by the Shock of a small Piece of harden'd Steel broken off from a steel Rod of about half a Quarter of an Inch in Thickness.

That of two Phials, which a Boy's playing Marble was dropped upon, one broke instantly, and the other not till three or four Minutes after.

That of the two Phials rubbed with the Finger on the Inside of the Hollow, the one did not break, but the other did, about five Minutes after.

That one of them did not break with the Fall of a rough *Cornish* Diamond into it, tho' the Experiment was twice repeated; but that the same afterwards was immediately broken by the Flint.

That the Trials made upon *English* Glasses, tho', seemingly, of the same Sort, did not succeed quite so well, and so certainly, as those made upon the foreign ones; which were, probably, more suddenly cooled, as the Workmen already acquainted with their Properties, may be more expert in their Management of them.

Upon the Whole, all the Company were perfectly satisfied of the Truth and Exactness of Monsieur *Allamand's* Experiments; and order'd their Thanks to be recorded both to that Gentleman and Mr. *Bentick*, for these very curious Communications.

Mr. *Allamand* observes, that he had yet only tried these Experiments upon Phials made of white or crystal Glass. But the *President* since received from the Reverend Dr. *Littleton*, F. R. S. some large hollow Cups, made at *Worcester*, of the common green Bottle Glass; all which, tho' of a much greater Size than the others, and some of them above three Inches thick at Bottom, were instantly broken with a Shiver of Flint weighing but about two Grains; tho' they had before resisted the Shock of a Musket-ball from the Height of near three Feet.

N. B. That all the foreign Glasses mentioned in this Paper were nearly of the Shape represented in TAB. I. *Fig. 1.* and about four Inches in Height.

IV. *Extract of a Letter from the Reverend Henry Miles, D. D. & F. R. S. to the President, relating to some Improvements which may be made in Cyder and Perry.*

The Letter read
Nov. 1745.

THE Design of communicating the following Paper to the *Royal Society* is, to invite Gentlemen, after the Example of a Practice that has long obtained in *Herefordshire*, to attempt an Improvement of their waste Lands, by planting such kind of Fruit trees, as are mentioned, in Hedges and barren Places; which, for aught appears, would thrive as well in other Counties, perhaps in some Parts of most Counties in *England*, as in that of *Hereford*.

Extract from a Manuscript, written Anno 1657-8, by Mr. afterwards, Dr. John Beale, & F. R. S. in the Way of an epistolary Address to S. Hartlib, Esq; for his Use, and that of Mr. Pell, the then British Resident at Zurich; and which appears to have been intended as a Sequel to that scarce and valuable Piece intitled Herefordshire Orchards, inserted in the later Editions of Mr. Bradley's New Improvement of Planting, &c.

Continuing

Fig. 2.
p. 541.

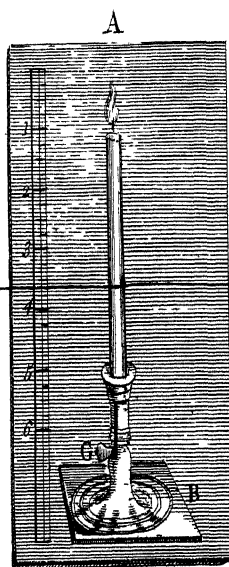


Fig. 3.
p. 556.

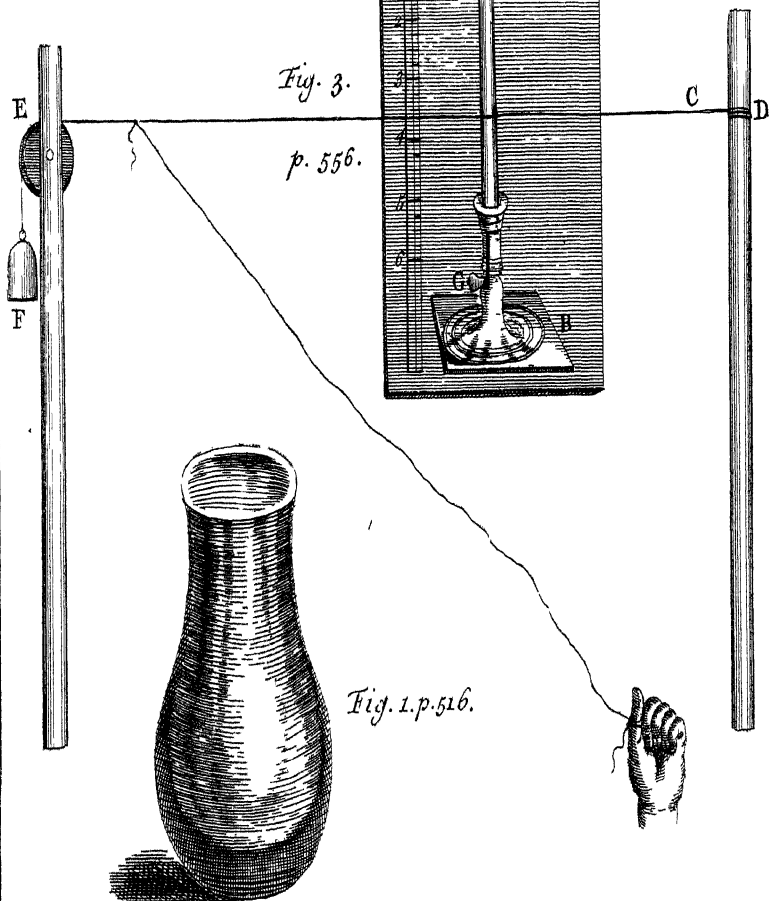
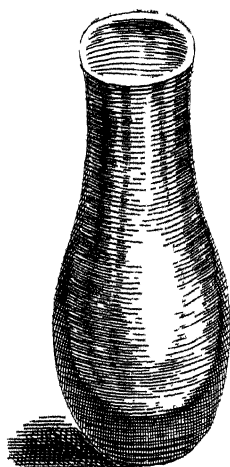


Fig. 1. p. 516.



GENIO ET HONOR
L POMPEI L F POLHEREN
NIANI EQ ROME Q PVB
Q AER PETALIM AEDIL
II VIRO CVRATORI
KALENDARIOR REI P
COLLEGIVM PASTO
PHORORVM INDVS
TRIENSIVM PATRO
NO OB MERITA

T · GRAE · TROPHIMVS · IND · FAC ·

*Concerning an excellent Liquor made of a Mixture
of rough Pears and Crabs.*

THE Author undertakes to evince, “ That Crabs and wild Pears, such as grow in the wildest and “ barren Clifts, and on Hills, do make the richest, “ strongest, the most pleasant and lasting Wines that “ *England* yet yields, or is ever like to yield.” — “ I have so well proved it already (says he) by so “ many hundred Experiments in *Herefordshire*, that “ wise Men tell me, that these Parts of *England* are “ some hundred thousand Pounds Sterling the better for the Knowledge of it.”

He mentions, of these Kinds of austere Fruit, the *Bareland* Pear and the *Bromsbury* Crab, of which Notice is taken Page 4th of the Tract intituled *Herefordshire Orchards*; and intimates, “ That tho’ the “ Discovery of them was but then lately made, yet “ they had gotten a great Reputation.” — He adds, “ The croft Crab and white or red Horse-pear do “ excel them, and all others, known or spoken of “ in other Countries.” Of the red Horse-pear of *Felton* or *Longland* he observes, “ That it has a “ pleasant masculine Vigour, especially in dry “ Grounds, and hath a peculiar Quality to overcome all Blasts.” — Of the Quality of the Fruits he says, “ That such is the Effect which the Austerity has upon the Mouth on tasting the Liquor, “ that the Rustics declare tis as if the Roof were fled away; and that “ neither Man, nor Beast, care “ to touch one of these Pears, tho’ never so ripe.” Of the Pear called *Immy-Winter*, which grows about *Rosse* (in that County) he observes, “ That it is of

“ no Use but for Cyder ; that if a Thief steal it, he
 “ would incur a speedy Vengeance ; it being a
 “ furious Purger, but, being joined with well chosen
 “ Crabs, and relieved to a due Maturity, becomes
 “ richer than a good *French* Wine ; but, if drank
 “ before the Time, it stupefies the Roof of the
 “ Mouth, assaults the Brain, and purgeth more vio-
 “ lently than a *Galenist*.” This Quality, he apprehends, will sufficiently secure the Fruit from being stolen, tho’ the Trees should be planted in the most remote Grounds.

Of the Quality of the Liquor he says, “ That, according as it is managed, it proves strong *Rhenish*,
 “ *Backrac*, yea pleasant *Canary*, sugar’d of itself,
 “ or, as rough as the fiercest *Greek* Wine, opening
 “ or binding, holding one, two, three, or more
 “ Years — that no Mortal can yet say at what Age
 “ it is past the best. This (adds he) we can say,
 “ that we have kept it till it burn as quickly as
 “ Sack, draws the Flame like *Naphtha*, and fires the
 “ Stomach like *Aqua Vitæ*.” He saith, “ That he
 “ made Trial at his own House with Wine d’*Hay*,
 “ by a Merchant of *Bristol* highly extoll’d, which,
 “ compar’d with a Liquor made of Crabs and wild
 “ Pears, was so much inferior, in the Judgment of
 “ all, that the Comparison was ridiculous.” And he further relates, “ That a Gentleman (Sir *H. Lingens*)
 “ a great Planter, and expert in many Experiments,
 “ had then by him many Tuns of a Liquor made
 “ with this Mixture of Fruit, which he, by a designed
 “ Equivocation, called Pearmain Cyder, that carried the Apple from all Palates — that all his
 “ common

“ common Hedges yielded him Store of the said
“ Fruit.”

To recommend this easiest, cheapest, and most profitable kind of Agriculture, (as he calls it) he says,
“ That the best of these Pears grow upon very bare
“ and sandy Hills, or Vales; Crabs on any Mound
“ or Bank that may be raised on an Heath; that one
“ Pear tree ordinarily bears yearly 40, 50, 60, 70
“ Gallons of Statute-Measure, and some 5, 6, or 7
“ times as much. Since I undertook this Argu-
“ ment (adds he) within 10 Miles of this Place we
“ made in one Year 50,000 Hogsheads, as I exa-
“ mined, not by Fancy, but by Rule and Inquiry;
“ and this shews the Hardiness of the Fruit. Let
“ our noble Patriots weigh, that this is not a
“ Thing in the Air, but a most certain and appa-
“ rent Truth, importing no less than the Art of
“ raising Store of rich Wines on our common
“ Arable, on our Hills, and waste Grounds; the
“ Charge a Trifle, the Pains very small, the Profit
“ incredible. Hence my Design is to urge the in-
“ credible Benefit that would redound to these Na-
“ tions, if leading Persons would make themselves,
“ their Tenants and Cottagers, all happy by follow-
“ ing our Example. I leave the Reader to cast up
“ how many Millions of Hogsheads of Wine, in a
“ few Years, would be raised in the Land. And truly
“ I conceive it the chief Cause, that, in all these Times
“ of late Wars, none of our poorest Cottages did see
“ Want; in all Houses they had the same Number
“ of Meals, and the same constant Fare: Our Ara-
“ ble seems not a Job the less, nor our Pasture the
“ less; and for some Uses the Shadow of the O-
“ chard

“ chard brings on the Grass a Fortnight the sooner,
 “ as commonly for Ewes and Lambs.”

The Author concludes his Tract with these Words,
 “ If this Discourse be duly valued, we need not
 “ raise Wars to destroy one another, or eat up one
 “ another, as we do ; in a short time we may be pro-
 “ vided of Fruit enough for another World as big
 “ as this, and to make this a true Paradise.”

V. *Extract of a Letter from Mr. Henry
 Baker, F. R. S. to Martin Folkes, Esq;
 President of the Royal Society, concerning
 the ancient Bridewell at Norwich.*

S I R,

Read Nov. 7. 1745. **I** SHALL beg the Liberty to give you
 some Part of a Letter I received lately
 from Mr. *William Arderon* of *Norwich*, which men-
 tions a remarkable Piece of Art as well as Antiquity
 remaining in that City.

The last Century (says he), and the present, may
 be allowed, I think to have produced more useful
 Discoveries than any five since the Creation : But the
 Lives and Abilities of us poor Mortals are so short
 and limited, that, whilst we are finding new Arts,
 others perhaps not less useful, slip away from us,
 and are lost. Nor would he deserve much less of
 Mankind, who can be so happy to restore the Know-
 ledge of a lost invention, than he that finds out a
 new one. And this of no little Service towards the
 Recovery

Recovery of lost Arts, to record that they *have been*, in order to make them again sought after and restor'd.

This City of *Norwich* affords a remarkable Instance of an Art we have now lost; I mean the wonderful Art, which our Ancestors knew, of cutting or rather breaking Flint Stones into uniform Figures, of equal Sizes, and with smooth and plain Surfaces. Many Remains of this sort are to be seen in our old Buildings; but none is more artificially and regularly finished than the North Wall of our *Bridewell*, which in Length is one hundred and fourteen Feet, and in Height thirty.

This ancient Structure, as all ingenious Strangers agree, is one of the greatest Curiosities of its kind, either in our City or County, and is not perhaps to be outdone in any Part of the known World *. It was built by *William Appleyard*, the first Mayor of *Norwich*, who 342 Years ago, that is, in the Year of Christ 1403, held his Mayoralty there: And, what is very strange, this Flint-work appears now as perfect as if it had been finished but Yesterday; whereas the Bricks, which were, after a certain Manner, wrought-in near the Bottom of the Wall, as a Ground-

* The Gate of the *Austin Friars* at *Canterbury*, that of *St. John's Abbey* at *Colchester*, and the Gate near *White-Hall*, *Westminster*, are in the same Taste. But the Platform on the Top of the *Royal Observatory* at *Paris*, which, instead of being leaved, is paved with Flint after this manner, is an Instance that the *French* have, in some measure, recovered this Art.

Ground work, are almost intirely rotted away. The Windows and Mouldings, which were built at the same time, of Freestone, are nearly in the same Condition. But these Flints have hitherto defied the devouring Teeth of Time, and will, probably, continue untouch'd for many Ages; being, perhaps, the most durable Way of Building that ever was yet invented.

These beautiful Flint stones are squared to such a Nicety, that the thin Edge of a Knife cannot be insinuated between the Joints without a great deal of Difficulty; and it is no easy Task to make out that they were laid with Lime. Most of them are about three Inches square, and as smooth and level as if they had been ground. They are also laid with such great Exactness, that no Brick-work, or Hewn-Stone, appears more regular in its Courses. I am,

S I R,

London, Octob. 30.
1745.

Your most obedient, and

Faithful humble Servant,

H. Baker.

VI. *Part of a Letter from the Reverend Mr. Geo. Costard to Mr. John Catlin, concerning a fiery Meteor seen in the Air on July 14. 1745.*

S I R,

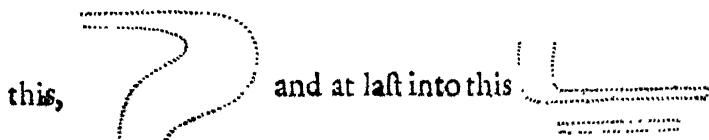
I have this Minute brought in to me the *Transaction*, N^o. 473, in which is an Account of a Phenomenon seen May the

the 27th, 1744; I shall now inform you of another, seen by myself on *Sunday, July* the 14th, 1745.

As I was coming from my Living, just before I reach'd a Place called *Stanlake broad*, and a little before 8 o' Clock in the Evening, I was on a sudden surpris'd to see a long Stream of Fire, of a Colour resembling molten Glass, and of a Figure like that in the Margin, which shot down from *A* to *B*, in Length, I guess, about twenty Degrees, and seem'd immediately to run up again from *B* to *A*; where it turned to a sort of Smoke, or rather to a fine lambent Flame like that of an *Aurora borealis*; which continu'd for some time in a sort of oblong Shape,



but afterwards by degrees, changed into



other Form, under which, parallel to the Horizon, it grew fainter and fainter, till it intirely vanish'd about nine o' Clock.

There was a fine gentle Breeze all this time; but I could not observe that it affected the *Phenomenon* so far as to make it change its Place, which

was to the Eastward of the North. Perhaps this Change of Figure might, in some measure, be owing to it. Being on Horseback, I saw it from the Beginning to the End; but having no Watch with me, I only guess'd, by my Riding, that it continued about an Hour. I heard afterwards, from some who had their Watches, that it lasted an Hour and one Minute.

When I came home, I put down what I had observed; it being, as I thought, a very singular Appearance. I am,

S I R,

Oxford, Aug. 2,
1745.

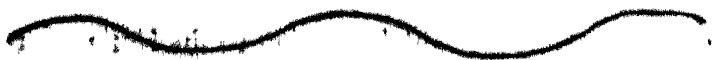
Your faithful

Humble Servant,

G. Costard.

VII.

As I was returning home from the *Royal Society* to *Westminster*, on *Thursday*, Dec. 16. 1742, h. 8. 40'. p. m. being about the Middle of the Parade in *St James's Park*, I saw a Light arise from behind the Trees and Houses in the S. by W. Point, which I took at first for a large Sky-Rocket, but when it had risen to the Height of about 20 Degrees, it took a Motion nearly parallel to the Horizon, but waved in this manner,



and went on to the N. by E. Point over the Houses. It seemed so to very near, that I thought it passed over *Queen's Square*, and then, as the Light rose the Clouds hid it out of sight of it over the Mountains. It continued so very long, that I had it above half

VII. *Extract of a Letter from Mr. Benj. Cook, F. R. S. to Mr. Peter Collinson, F. R. S. concerning the Effect which the Farina of the Blossoms of different Sorts of Apple-trees had on the Fruit of a neighbouring Tree.*

Dear Cousin, Newport (Isle of Wight), Oct. 1745.

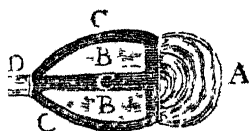
The Apples shown
Nov. 14. 1745.

I HAVE sent you some *Russetings* changed by the *Farina* of a next-door Neighbour, whose Name I wanted Skill to know; but can only say, that the *Russeting* has exactly acquired his Face and Complexion.

[Mr. Collinson then produced several Samples of the Apples; an unteinted Russeting; a Russeting

half a Minute in View; and therefore had Time enough to contemplate its Appearance fully, which was what is seen in the annexed Figure,

E



A seemed to be a light Flame, turning backwards from the Resistance the Air made to it. *B B* a bright Fire like burning Charcoal, inclosed as it were in an open Case, of which the Frame *C C C* was quite opaque, like Bands of Iron. At *D* issued forth a Train or Tail of light Flame, more bright at *D*, and growing gradually fainter at *E*, so as to be transparent more than half its Length. The Head seemed about half a Degree in Diameter, the Tail near 3 Degrees in Length, and about one Eighth of a Degree in Thickness.

C. M.

ring changed in Complexion, which grew among a great Cluster of unalter'd Brethren; and some Apples of the other Tree, which had caused the Change in the Russetings, and whole Fruit had in Return received a rough Coat from the Russetings.]

Theophrastus takes notice of this Παράλλαξη, as he calls it; and tells us the old Divines were wont to make a great porther about it, and foretel great Events by it: *Pliny* informs us, there was one who wrote a whole Book about such Changes. But the Use I should make of it, is chiefly this, that it may be of Importance to the Curious in Fruits, to take care how their Trees are sorted, and what Company they keep. For tho' this Change be not so conspicuous in Apples which have a smooth green Coat, as in the Russet-breed, yet one may suppose Impressions of this sort often made on them; and perhaps their Juices alter'd for the better or worse.

* * *

Tours, &c.

B. Cooke.

VIII.

Mr. Sir Jos. Ayloffe, a worthy Member of this Society, communicated, on July 1. 1731. from the Reverend Mr. Henchman, Parson of Salisbury, some Observations of Pease of different Colours issuing one another in the same manner as the Apples above-mentioned.

Mr. Henchman, in the Spring 1729. sowed a Piece of Ground in his Garden with white Pease, and two double Rows of blue Pease, between which were four rows of white; in Autumn, upon gathering some

VIII. *Some Account of the Sinking down of a Piece of Ground, at Horsford, in Norfolk; communicated by Mr. Arderon, of Norwich, to Mr. Henry Baker, F. R. S.*

Read Nov. 14. 1745.

IN the Night-time, between the 24th and 25th of *June* last past, a violent Storm of Thunder and Lightning happened at the City of *Norwich*, and the Places adjacent; tho' at the City of *Norwich* it seemed extraordinary only for the Loudness of its Claps, and the Length of several of the Flashes; some whereof continued near half a Minute, and were so extremely bright, that they caused some thin Deal Shutters to the Windows of my Bed-Room (which then happened to be unpainted) to appear almost quite transparent.

But at *Horsford*, a small Country Village, about four Miles North-west of this City, a remarkable
Phænomenon

some for Seed, he opened one of the Pods, and was surpris'd to see one blue Pea at the End next the Stalk, with six white Pease: But after having examined several other Shells very carefully, he found a great Variety of Intermixtures of the white and blue Pease in the same Shells; sometimes one white (or blue) only at one End, sometimes at each End; sometimes two white (or blue) with one of the other Colour interchangeably; and thus the whole Parcel that was rubbed out for Seed was intermixt white and blue. The next Year, he says, not having Plotts of white and blue Pease standing near one another, he did not find any such Mixture in the several Parcels then sowed for Seed. But it is pity he did not pick out a sufficient Number of the blue Pease from among the white, and sow them by themselves, in order to see what colour'd Pease this mixt Breed would have produced,

C. M.

Phænomenon appeared the next Day, the like whereof has not been observed in this County, since that communicated to the *Royal Society* by Mr. P. Le Neve, as in the *Phil. Trans.* N^o. 355.

A sudden *Lapsus*, or Sinking down of the Earth, happened at this Village, in the Night above-mentioned, and left a Hole twelve Feet deep, and twelve and half in Diameter. in Form almost exactly round. Its Sides are nearly perpendicular; and what seems most strange, no Ruffles, Cracks, or Chasms, are to be found nigh it, but the Ground appears intirely firm and solid; and for Miles about is a fine Champaign Country, of a dry sandy Soil, but not hilly; neither is there any Watercourse above Ground near it.

The first Yard from the Surface downwards is Corn-nould Earth; the other three are composed of brown and yellow Sand, disposed in several different *Strata*.

I shall not pretend to account for this Accident; but might it not possibly be occasioned by some subterraneous Current washing away the sandy Matter by little and little, until it had left only a Crust, which the *Tremor* of this terrible Thunder had thrown down from the very Surface; though on this Conjecture, one would expect some Overflow or Appearance of Water, whereas I could not perceive here the Remains of a single Drop.

IX. *A Letter from Mr. James Simon to the President, concerning the Bones of a Fœtus voided per Anum; and of some Fossils found in Ireland.*

Read Nov. 14.
1745.

A Curious and worthy Clergyman, of the County of *Armagh*, sent me, some time ago, a Parcel of Bones, with the following Account of them; *viz.*

“ *Rose*, the Wife of *Mortaugh Mac Cornwall*,
 “ of the Parish of *Tullylish*, Barony of *Clare*, in
 “ the Year 1741, about the latter End of *May*, or
 “ the Beginning of *June*, being in the 37th Year
 “ of her Age, and Mother of several Children, con-
 “ ceived, as usual; but, in two or three Days after,
 “ felt an excessive unnatural kind of Pain in the
 “ *Matrix*, which continued, with frequent Faint-
 “ ings, a depraved Appetite, and an exceeding great
 “ Weakness, till her Child quickened; after which
 “ she proceeded reasonably well in her Pregnancy to
 “ the End of nine Months; and then, her Child
 “ alive, and every thing right (as the Midwife
 “ thought); she fell in Labour, which lasted, with
 “ proper Child bearing Pains, for twenty-four Hours,
 “ but could not be delivered; and her Labour leav-
 “ ing her, the Child was no more observed to stir.
 “ In a Month after, her Labour return’d, and, with
 “ many regular Throws, continued twenty-four
 “ Hours more, but to no Purpose, save the dis-
 “ charging of some Quantities of black corrupted
 “ Clods of Blood; of which kind also she threw
 “ up

“ up much by Vomit: Then her Labour left her
 “ intirely; and soon after she felt the decaying of
 “ the Flesh of her Infant, and the Discharge thereof
 “ both by the *Matrix* and *Anus*, with so putrid and
 “ deadly a Smell as was extremely nauseous both to
 “ herself and others about her. — Thus she lived
 “ for upwards of twelve Months, and, at that Pe-
 “ riod her Pains increasing to Excess, she began the
 “ discharging of the Bones, which, to the Number
 “ of 80 and upwards, she voided wholly by Siege;
 “ 1st the first Day, and 2, 3, or 4, at a time alter-
 “ wards for the Space of twelve Months or more,
 “ with most intolerable Pains at the voiding of each
 “ Bone, especially a broad Piece of the Skull: So
 “ that, from her Conception to the Day of her
 “ Death, which was the 4th of *April* last, makes up
 “ near four Years; during most of which Time,
 “ never was a more calamitous Creature: For three
 “ Years, scarce a Day without suffering most exqui-
 “ site Torture, being also attended with frequent
 “ Faintings, a continual Want of Appetite, and an
 “ almost perpetual Looseness; insomuch that it is
 “ miraculous how she lived, not eating all that long
 “ Space so much as would have sustained a sucking
 “ Infant; even the very Liquids, at length, not lying
 “ a Moment in her Stomach, by which means she
 “ became quite emaciated, and dismal to look at,
 “ not being able to move from one Posture to an-
 “ other, or to be moved, without fainting at every
 “ the least Touch or Motion. The Truth of all
 “ which I attest to you, as I received it partly from
 “ the poor Woman herself, and also from my Wife,
 “ who was present frequently during her Illness.”

I thought, Sir, that this Account might be agreeable to you, and to the *Royal Society* in general."

In my little Excursions in Quest of Fossils in this County, I found, the 13th Instant, what Naturalists call *Lac Lunæ*; but think Dr. *Plot* is mistaken, when he gives it as a Criterion or Sign of good Lime-Stone; for the two Quarries where I found it were building Stone, but will not burn into Lime. This Matter or Earth makes a strong Ebullition with Vinegar and Spirit of Vitriol. Some of it was as soft as Cream-Cheese, when I took it out of the Fissure of the Rock; the other was hard, some in thin Crusts, and some in pretty thick Lumps. It never was taken notice of in this Kingdom before. I also found, about six Weeks ago, white native Vitriol, which I take to be the *Capillaris* Sort; but as we have no Naturalist here, nor Collection of Fossils, or any other natural Curiosities (tho' in great Plenty in this Kingdom), it is hard for me to give Names to such as I have (about 800 Articles) or do discover daily.

S I R,

Dublin, Sept. 175
1745.

Your most humble, and

most obedient Servant,

James Simon.

•

X. *Some Account of the Distemper raging among the Cow-kind in the Neighbourhood of London, together with some Remedies proposed for their Recovery by Cromwell Mortimer, M. D. Secr. R. S. and Fellow of the College of Physicians, London.*

Read Nov. 21. 1745. **T**Hinking it my Duty, as a Physician, to contribute my small Mite towards remedying this publick Calamity, I have been at the Trouble of visiting several of the Cow-houses near the Skirts of *Westminster* and *London*, where I have collected what Intimations I could from Persons who have been conversant with the Cows during the whole Course of their Illness, have carefully examined several sick Cows myself, and have seen two open'd.

The great *Hippocrates* did not think it beneath him to consider the Distempers of Horses, and has left us an excellent Treatise on that Subject; surely then it cannot derogate from the Dignity of the Profession now-adays for the most eminent in it to lend their compassionate Assistance to any of the brute Creation; especially to such Creatures as are more immediately of Use, or even Pleasure, to Man: And as the Price of Horses and Dogs has of late Years been raised to most extravagant Rates, it is a Wonder the Owners of them have never been so generous as to encourage Gentlemen of higher Degrees of Learning than the Farmer and the Cowleech to make themselves acquainted with the Diseases of Horses, Cows, and other Cattle, and to try Methods for their Relief

Mr.

Mr. *Bates*, a worthy Member of this *Society*, has given us a very particular Account of the Sickneſs among the Cows in 1714. (ſee *Phil. Tranſ.* N°. 358.) but he has omitted one Circumſtance I remember myſelf, having ſeen many Cows die near *Bethnal-Green*; they commonly came to the Ponds to drink, were taken giddy, fell down, were convulſed, bled much at the Noſe and Mouth, and ſo died.

Dr. *Lobb*, a very diligent and laborious Obſerver of what occurs in his Profeſſion, as his Hiſtories of various Caſes of the Small Pox, and his curious Experiments on Diſſolvents of the Stone, ſufficiently evince, has lately publiſhed, in a Collection of Letters relating to the Plague, an Inquiry into the Quality of the Cauſe of the contagious Sickneſs among the Cattle: It were to be wiſh'd what he propoſes were now tried *.

As to the Diſtemper now reigning among the Cattle, I am informed by the Cowkeepers, that a Cow ſhall be ſeemingly well, and feed heartily over night, or in the Morning, and give the uſual Quantity of Milk; that in twelve Hours time they ſhall all of a ſudden abate in their Milk near half, and intirely fall off their Stomach, ſo as neither to eat or drink, and then gradually loſe all their Milk. As ſoon as they perceive this, they give them a warm Maſh of Malt, or the following Drench: “ Take two Ounces of
“ Caraway-ſeeds, boil them in a Quart of Water,
“ and ſtrain it; add a Gill of White-wine, and a
“ Quarter of a Pound of Honey.”

· Their

* See his Letter to *John Milner* Eſq, firſt *Commiſſioner* for examining the State of the *Diſtemper* among the Cows.

Their Teeth are commonly observed to be loose; for which they lance the Gums, and rub them with Salt and Vinegar.

The very first Day they have a Huskiness, breathe short, and wheeze, but have no great Cough; for which they have blooded them, [in too small Quantities] and rubb'd their Noses with Tar, but with no Success.

Some hang down their Heads, and run much at the Nose; for which they lay a Bag of scalding hot Malt to their Heads, tying it between their Horns. This has sometimes relieved this Symptom, but the Beasts have not recovered.

The second or third Day most of them, not all, fall into a Purging, groan much, and seem to be in great Pain. The Stools, I have seen, seem to be bilious, have Cakes of Jelly come away with them, and some were streaked with Blood. They soon die after these Stools come on.

Those that are kept out in the cold Air seldom live beyond the third Day; those that are kept warm in Houses, and cloathed, live five, six, or seven Days.

Many of the Cows, I have seen, have a wild Stare with their Eyes; the Whites of the Eye, and the Skin of the Eye-lids, look'd yellowish: Their Tongues look'd white; they had no extraordinary Heat in their Mouths; at the Roots of their Horns (a Place where they usually feel to judge of the Heat of Cattle), or in the *Maxilla* or Arm-pit, if I may so call it. The Mucus running from their Nose is very thick and ropy: Their Milk is thick and yellow.

In the two I have seen open'd, the Flesh and Blood look'd much darker colour'd than usual; the Fat of the Livers look'd yellow; the Lungs were much inflamed in many Places, and had several large Blisters.

two or three Inches over, full of Water, on their outward Surface: There was no Water in the *Thorax*, little or none in the *Pericardium*: The Heart look'd well, but the Blood in it was not at all clodded, being exceeding fluid and dark-colour'd: The Paunch was very full of Food, and greatly distended: The Stomach look'd well; the Liver was full of scirrhus Swellings and chalky Knobs; the Gall-bladder bigger than usual; the Gall fluid, but dark-colour'd; the Intestines inflamed in many Places; the Fat about the Kidneys was distended with Air; the Kidneys were sound, as was the Bladder and *Uterus*. This Cow was not with Calf. On opening the Scull there was much Water gushed out.

In the second Cow the Fat was not yellow; the Lungs, Heart, Paunch, and Stomach, were like the former; the Liver was pale, flabby, not scirrhus; but the Gall-bladder very large; the Intestines inflamed, and in some Places livid; the Fat of the Kidneys in this was sound, but one of the Kidneys was mortified. This Cow was about a Month gone with Calf.

The Man who flea'd and open'd these Cows said, These were the general Appearances in most he had flea'd; only that in some he found Water in the Cells of the Cores of the Horns.

They flea off the Hides, which they say are good to tan; and they save the Fat to make Tallow of. The Fleaer told me, a poor Man, made a hearty Meal of some Steaks he cut off one of these Cows, and that he was not sick with it*.

From

* I am assured, that a very sufficient Experiment was made in our Army in Flanders last Campaign in Favour of this.

From these Circumstances I think it evident, that this Distemper begins by an Inflammation of the Lungs, attended with a Catarrh or Flux of Humours from the Nose; that in the Progress of it there comes on an Inflammation of the Guts, and a Purging, caused by an Acrimony and Overflowing of the Gall, which ends in Stools tinged with Blood, exciting great Pain in the Bowels, and so brings on Death.

Bleeding (in small Quantities) has not been found effectual, nor in short any of the Remedies yet made use of; therefore, having a chief Regard to the ultimate Effort of Nature, which seems to be to carry off the Distemper by an extraordinary Discharge of Gall, I hope the Use of *Crocus Metallorum*, a Medicine made use of with Success in Horses, and a great Discharger of Gall, as I have known its good Effects in the Jaundice in Men, may be attended with Success: I have therefore propos'd to some Cow-keepers to give to a Cow, as soon as taken ill, one of the following Balls.

- “ Take *Crocus Metallorum* half an Ounce* in
- “ Powder; make it into a Ball with Dough or
- “ Crum of Bread moisten'd; give the Cow a
- “ Draught of Bran and warm Water after it, and
- “ repeat the Draught after every purging Stool.”

For the Running at the Nose, I am told, that pouring a Pint of warm Vinegar, with an Ounce of Salt, into the Nostrials, has proved successful in making the Cow sneeze, and discharge a great Quantity of thick yellow *Mucus*, and other Matter, from the Nose, after which the Cow recover'd.

For

According to the Size and Strength of the Cow; or as

For the Shortness of Breath, I have advised the giving “ Whale-Oil, Treacle of Sugar, each a Pint; “ Flower of Brimstone four Ounces: Give it in a “ Mash of Malt, or Bran and Water, twice or “ thrice a Day.”

For the Scouring, first give the *Crocus*-Purge above-mention’d; then give them every six or eight Hours the following Draught.

“ Take Whiting one Pound, bruise it; pour
“ boiling Water upon it, a Quart or more; let
“ it stand to settle; pour off the clear Water,
“ and fling it away; then put a Quart of warm
“ Water to the wet Whiting; and add Bole-
“ *Armeniac* in Powder two Ounces, *Venice*-
“ Treacle one Ounce, *English* Malt-Spirits half
“ a Pint.”

These Proposals being founded upon the Appearance of the Symptoms, I hope they will be attended with the wish’d-for Success.

XI. *A Letter from Mr. D. P. Layard, Surgeon, to C. Mortimer, M. D. Secr. R. S. inclosing an Account of a Fracture of the Os Ilium, and its Cure.*

S I R,

Read Dec. 5. 1745. I Nclosed I send you a Case, which, as the Learned *Heister* observes, *rarely happens.*
Z z z

happens *. If you think it may be worthy the Notice of so distinguished a Body as the *Royal Society*, I beg the Favour of you, Sir, to present it, as a small Token of my great Respect. I am, with the greatest Regard,

S I R,

Dean-Street, Soho, Dec. 3.
1745.

Your most humble,

and affectionate Servant,

D. P. Layard.

The Case of John Easdon, Coachman to the Right Honourable the Earl of Darnley.

ON the 8th of February 1745, John Easdon, Coachman to the Right Honourable the Earl of Darnley, about 22 Years of Age, was jammed between a Waggon and a Coal-Cart; as he was getting up into the Waggon, the Cart-Wheel pressed on the upper Part of the left *Os Ilium*, and, by a sudden Jolt, squeezed him against the Waggon, so as to raise him from the Waggon-Wheel on which he stood; then the Cart going on, the poor Man fell on the Ground.

Being carried to my Lord's Stables, I examined the Part, and found (just below the Contusion made by the

Commotionem non frangitur. Heister Instit. Chirurgic. Tom.

the Pressure of the Cart-Wheel) a Fracture running quite across the *Costa* of the left *Os Ilium*, about three Fingers Breadth below the *Crista* of the said Bone; the End of the upper fractur'd Part being forced in towards the Cavity of the *Abdomen*.

The Patient being laid on his Back, on the Edge of the Bed, I applied a Napkin on the *false Ribs*, which was pulled tight by two Assistants, in order to press the Contents of the *Abdomen* downwards: Another Assistant pressed the *Abdomen* on the right Side, while, by pressing the *Crista* of the fractured *Os Ilium* gently inwards, I brought both the Edges of the Fracture to a mutual Contact.

The Fracture being reduced, the Napkin applied on the *false Ribs* was tightened, and kept on during the whole Cure with the *Scapulary*. I applied proper Compresses, and a Pasteboard cut according to the Figure of the Bone, over which I applied the * *Spica Bandage*. The Patient was kept in Bed for about three Weeks, lying on his Back, the affected Side being supported with a soft Pillow. By this means, and by observing what is generally recommended in all Fractures, the Patient was perfectly cured; and walked very well at the Month's End.

* *Vide* Heister. *Institut. Chirurgic. Tom. 2. pag. 1217.*

XII. *Some Account of a curious Tripas and Inscription found near Turin, serving to discover the true Situation of the ancient City Industria.* By David Erskine Baker.

This is an Abstract of the Paper, read Dec. 5. 1745 **D**R. Joseph Laurentius Brunus, Fellow of our Royal Society, and Physician of the College at *Turin*, having, in the Month of *March*, 1744-5, sent from thence to my Father the Description of a most curious antique *Tripas* of Metal, found, some little while before, together with a Plate of the same, bearing an extraordinary Inscription thereon, at a Village call'd *Monteu*, on the right Side of the River *Po*, about 16 Miles from *Turin*; and the same Gentleman having lately sent us likewise an *Italian* Dissertation printed at *Turin*, wherein the learned Authors (*Paul Ricolwi* and *Anthony Rivautella*) undertake to discover, from the said Inscription, and other concurring Circumstances, the true Place of the ancient City *Industria*, mentioned twice by *Pliny*; a short Account, collected from the Whole, and translated into *English*, may prove, I hope, not unacceptable.

This *Tripas*, they say, far exceeds every thing of its Kind, preserved hitherto in any of the Cabinets in Europe, as well for its Structure, as for the Variety and Elegance of the several *Relievo's* where-with it is adorned. Each of its three Pillars has on it four small Figures: The first, which is placed at Top, represents *Tannius* of *Venus*; the second is a *Victory*.

Victory, or a winged *Fortune* rather, standing with her Feet upon a Globe; the third, which is near the Middle of the Pillar, is an Harpy, winged, with a Woman's Face; and the fourth Figure, at the Foot of the Pillar, appears to be an old *Silenus* or Satyr, crouching himself together in an odd Manner.

The Pillars are joined to one another by little Bars of Metal, fastened by Rivets at Top, and Rings at Bottom, in such a manner that they may be closed together, or drawn asunder, at Pleasure; and when they are extended to the utmost, the Size of the *Tripes* is somewhat more than a *Turin* Foot, which, Dr. *Bruni* says, is equal to twenty *English* Inches.

TABLE I. *Fig. 2.* is an exact Copy of the Inscription on the Metal Plate, as given before the Dissertation. The Authors explain the Reading of each Word, and give their Comments thereon, together with a long Description of the *Roman* Customs and Offices; which being sufficiently known, I shall take from them only a brief Abstract of such Passages as are least easy to be understood.

The first Observation of this kind is, that whereas, in other Inscriptions, it is usual to find *Honori*, and then the Name of the Person in the Dative Case, as *Honori Memmio Vitrasio Orfito(a)*; or else, *Honori Imperatoris Caesaris*, &c.; or as in another Stone (b) *in Honorem T. Claudii Imperatoris*; and consequently the Titles of the Person in the Genitive Case; here, after having said, *Honori L. Pompei Herenniani*, we find *Curatori* and *Patrono*, which two Words must again

(a) Grut. p. 443.

(b) Marm. Taur. p. 189,

again refer to the Words *Genio & Honori*. And they mention another Inscription, where a Mistake of the same kind is found; *viz.* (a)

HONORI
M. GAVI. M. F.
POP. S. Q. V. I. T. A. N. I.
EQ. P. V. B. I. I. I. V. I. R. I. D.
I. I. I. V. I. R. A. P. V. B.
C. V. R. A. T. O. R. I. A. V. I. C. I. T. I. N. O. R.
A. P. P. A. R. I. T. O. R. E. S. E. T.
L. I. M. O. C. I. N. C. T. I.
T. R. I. B. U. N. A. L. I. S. L. I. V. S.
EQ. ROM. EQ. P. V. B.

These Words infer, that *Lucius Pompeius*, the Person to whose Honour this Plate is inscrib'd, was a *Roman Knight*, who had a Stipend from the Public. The *Roman Knights* served at their own Expence till the Year of *Rome* 451, when their Horses began first to be maintain'd at the Expence of the Commonwealth; and it appears, from various Inscriptions under the Emperors, that the Words, *Equus publicus*, *Equo publico donatus*, or *ornatus*, &c. always mean a military Dignity, and must be distinguished from the *Roman Knights* towards the End of the Commonwealth, who were a Degree of Citizens between the *Senators* and the *Plebeians*.

Q. AER. PET. ALIM.

It

It appears hereby that *Lucius Pompeius* was *Quæstor Ærarii*, tho' only of the Finances of the City *Industria*, and not of the Emperor under whom he liv'd. But the greatest Difficulty arises from PET. ALIM. where our Authors suppose the Engraver may have left out the Stop between the Letter P and the Letters ET; so that we should read it, *Quæstoris Ærarii publici et Alimentorum*; and then we have two different Dignities of *Lucius Pompeius*; that of *Quæstor* of the public-Taxes, and that of *Quæstor* of the Provisions, both regarding the City of *Industria*. Several other Inscriptions are also produc'd, to prove the Office of *Quæstor Alimentorum*; and a great deal of Reading is introduc'd, to shew, that the *Quæstor Alimentorum* was sometimes understood to be an Officer having the Care of the public Allowance for bringing up Children; and that at other Times his Office was understood to be the procuring all Sorts of Provisions for the Use of the Emperor's Troops.

Passing by his Office of *Ædis* and *Duumvir*, we find he presided likewise over the Receipt of the Taxes, by this Address to him,

CVRATORI
KALENDARIORVM. REI. P:

The Days fixed for Payment of the Taxes and Debts were register'd in the public *Calendars*; and Creditors usually demanded their Interest on the *Kalends*, or first Day of every Month: Whence the Register of the Debtors, and the Sums due, or the Tribute to be paid by Particulars to the Public, and indeed the
general

general State of the Debts, and Credit of every Community came to be called *Calendarium*.

COLLEGIUM PASTOPHORORVM

The College of Priests called *Pastophori*, a Name taken, as some suppose, from a very rich and ornamental upper Garment termed *Pastos*. As *Pastophorus* was a Name given to *Venus*, these Priests may have belonged to her, or else to the Goddess *Isis*, whose chief Priests, as *Lucius Apuleius* informs us, were called *Pastophori*, by way of Pre-eminence, *Unus — cætu Pastophorum, quod sacrosancti Collegii nomen est, velut in concionem vocato, indidem de sublimo suggestu — renunciat, &c. (a)* He says also, that the God *Osiris* had a College of them. His Words are, *Osiris — in Collegium me Pastophorum suorum, imo inter ipsos Decurionum Quinquenales elegit*. This Body of Priests had various Offices, one whereof might probably be the conferring Honours on Persons of great Merit, as we find from our Inscription those of *Industria* had done on *Lucius Pompeius*.

INDVSTRIENSIVM PATRONO OB. MERITA

Hereby the City *Industria* acknowledges *Lucius Pompeius* as its Patron, or Protector, and shews its Gratitude



Gratitude for singular Benefits received. Patrons and Clients were in the earliest Times of the Commonwealth; but, under the Emperors, Inscriptions shew us frequently, that Cities and Nations chose for their Patron some eminent *Roman* Citizen in Favour with the Prince, on whom they often conferred great Honours.

T. GRAE. TROPHIMVS. IND. FAC.

These Words on the Cornice our Authors wondered to find, as it was unusual for any but the most eminent Painters and Sculptors to put their Names, and that only to the most famous and perfect of their Works. Whence they conjecture, that this *Titus Graecus Trophimus* of *Industria*, might be not only the Engraver of the Inscription, but likewise the Sculptor of some Image to which this may have been the Pedestal.

These Gentlemen, who are Authors of the *Marmora Taurinensia*, went to this Village of *Monteu* in the Autumn of the Year 1743. where they found many Inscriptions, with the Names of various Magistrates both civil and ecclesiastical; which were certain Proofs that some considerable City had been in that Place formerly: And returning thither the Autumn following, they found a broken Stone; whereon, by putting the Pieces together, they could plainly read, that there had been decreed to a Person named *Cocceia*, at the Expence of the Public, a Statue AB. IND. which they interpret *Ab Industriensibus*, and suppose to mean the Citizens of *Industria*.

..... COCCILE.....

HA... LC... AB. IND.

..... FVNERE. PV.

H. S. I. A. I. V. A. M.

Some Peasants about the same time, digging in the Plain between the Hills near the *Po*, discovered the *Vestiges* of an ancient Brick, with some Medals; and, in the Middle of the following *February*, found the Traces of a large Room, other Medals, and some Pieces of wrought Brass; and in *March* they discovered this Plate and *Tripes*. Our Authors mention also the Discovery of many Medals, a mosaic Pavement, the Remains of an ancient Temple, *Basso-Relievo's*, little Images, Ruins of Edifices, and Inscriptions found here; and give two Passages from *Pliny*, one whereof is (a), *Ab altero (Apennini) latere, ad Padum amnem Italiae ditissimum, omnia nobilibus oppidis nitent; Libarnia, Dertonia, Colonia, Iria, Bardarate, INDUSTRIA*: In the other his Words are, (b) *Metrodorus dicit, quoniam circa fontem arbor multa sit picea, quæ Pades Gallice vocetur, Padum hoc nomen accepisse; Ligurum quidem linguâ amnem ipsum Bodincum vocari, quod significet fundo carentem. Cui argumento adest oppidum INDUSTRIA, vetusto nomine Bodincomagum, ubi præcipua altitudo incipit.*

In the first of these Quotations the City *Industria* is spoken of, as one of the noble Cities that flourished in the Time along the Banks of the River *Po*,
a little

a little Way to the South Side of the *Apennines*. In the other he explains himself more fully, describing it to be near the *Po*, where that River begins to acquire a greater Depth; and, as a Confirmation, gives its Name still ancients than that of *Industria*, viz. *Bodincomagus*, signifying in the *Ligustine* Tongue the River's being deeper at that Place. And our Authors affirm, that, even at this Day, the *Po*, above and near *Turin*, is hardly navigable; but at *Monteu*, after having received not only the *Dora*, but the *Stura*, the *Orco*, the *Mallone*, and the *Dora Balteo*, it becomes much larger both in Depth and Width. They also take notice, that the Hill near the Plain of *Monteu* is called *Mondicoi*, which they suppose a corrupted Remain of the ancient Word *Bodincomagus*. They find likewise, in the Bulls of this Parish, that the parochial Church is called *Sancti Joannis Baptiste de Lustria*; which, they conjecture, may, by Length of Time, have been formed from the ancient Name *Industria*.

From all these Circumstances put together, they seem confident of their having discovered the real Spot where this ancient City stood; and bring several Reasons to prove, that *Casal* cannot possibly be the Place, as some Writers have imagined; and in order to shew more fully the Grandeur, Magnificence, and Antiquity of this ancient City, they add the following Inscriptions found at the same Place.

A. HOSTILIO. A F
PAP. PATRONO.

[548]

C. AVILIO. L. F.
P. GAVIANO.
FLAMINI DIVI
CAESARIS
PIRPLEVO
PATRONO MVNICIPI.
TRIB. MILIT. LEG. III
GALLICAE
D. D.
QVO. HONORE. CONTENVS
IMPENSAM. REMISIT.

N. MINIO. A. F. POL
..... ANNIVS
PRIM... IRO. PRÆF. EQ.
COH. VI. Q.
HONORIS. CAVSSA. LOCVS
EX. D. D. DATVS. V. F.
SIBI. ET.
MINIÆ. ANN. F. ET. TVLLIÆ
VXSORI.

C. LOLLIO
C. LIB. PAL.
AGRAVLO.
COLLEG.
CENTONAR.
Q. H. C. I. R.

L. FVL FENIVS. T. F. SIBI
ET. L. FVL FENIO. L. F. SECVNDQ
FVL FENIO. L. F. SECVNDQ
FVL FENIO. L. F. SECVNDQ

T. SIBI
EIO.P.F.NIGRIN.
V. F.

IMP.CÆSARO
AVGVSTO
D. D.

The last Inscription proves the great Antiquity of this City.

XIII. *Further Observations on the Distemper now raging among the Cow-Kind, by the Publisher of these Transactions.*

Read Dec. 12. ^{1745.} SINCE my former Paper on this Subject (*Nov. 21. **) I have had Opportunities of being present when three Cows have been flea'd and open'd; the Lungs in all were inflamed and blistered, and the Guts in some Places inflamed, in others livid, the Gall-bladders exceeding large: A Collar-Maker's Man, who has been assisting in flea-ing above a hundred dead Cows, assures me, these are the general Appearances in them all; except that in one he met with a large Bag full of Corruption, between the Bag inclosing the Heart and the Backbone; in another he found the Gall-bladder quite contracted and shrivell'd up, having little or no Gall in it; and in several he found scirrhus Knobs in the Livers.

* See p. 532. *supra*.

Nov. 26. I desired Mr. *Hill*, an ingenious Apothecary in *Westminster*, to accompany me to see a Cow dissected, and to help me examine every thing very carefully, having got her drawn into a Shed, to defend us from the Weather.

When the Skin was taken off, she appeared very fat; the *Muscles* look'd of a darker Colour than usual. On opening the *Abdomen* the *Caul* appeared very fat; the *Paunch* was greatly distended; on making a Puncture much Wind pushed out: It had in it a great deal of Food; the Inside look'd well, and did not peel; the second and third Stomach, or the *Omasum*, as also the fourth Stomach or *Abomasum*, were almost empty, but looked well; the Liver was firm, well-coloured, and sound, except a few scirrhous Knobs about the Size of Nutmegs: The Gall-bladder was exceeding large, and full of very fluid Gall; the Guts were inflamed in many Places, the *Colon* and *Cecum* livid: I had the Curiosity to have them measured; from the *Anus* to the Insertion of the *Cecum* there were twelve Yards (the *Cecum* was an Ell long), and from the *Cecum* to the *Pylorus* there were fifty-two Yards. The Midriff was much swelled and inflamed: The Lungs were swelled, inflamed, adhered in some Places to the *Pleura*, and almost wholly covered with Bladders of Water: There was no Appearance of any Inflammation on the *Pleura*, or in either the internal or external intercostal Muscles: The Windpipe was inflamed greatly throughout its whole Course, especially its Inside; but the Gullet, which lay so near it, was not in the least inflamed: The Heart was of its natural Size, the *Pericardium* full of very fluid Blood, probably from the bursting of

of some Branch of the coronary Artery, caused by the extraordinary Accumulation of Blood in the right Ventricle; for the *Vena cava*, and right Ventricle of the Heart, were turgid, and full of black coagulated Blood, tho' this Cow had been dead but 12 or 14 Hours; the Lungs were likewise turgid with Blood, but little or none was found in the left Ventricle or *Aorta*; the Obstruction seemed to have been so great in the Lungs, that very little Blood could pass thro' them from the right to the left Ventricle of the Heart, and therefore evidently evinces the Existence of a confirmed Peripneumony. All the Membranes lining the Nostrils, and the spongy Bones thereof, were quite turgid with Blood, and in the highest State of Inflammation. The greater and lesser Brain looked fair and well, seeming no way distemper'd.

I have not seen, in any Cows I have examined, any cutaneous Sores or Exulcerations, nothing like the Boils, Carbuncles, &c. described by Authors as the constant Concomitants of the Plague in Men: Nor does there seem to be any Attempt of Nature to sling off the Distemper by any internal Impostumation, or Discharge, unless by the Running at the Nose, and by the bilious Stools, or bilious Urine. The few, which have recovered, have been such as have been kept within Doors very warm, have been blooded once, twice, or oftener, have had warm Mashcs of Malt and Bran given them, and warm Drenches of warm Herbs, such as Rosemary, Wormwood, and Ground-ivy, with Honey or Treacle, and have neither purged at all, or but little; and when they have

not

not purged at all, their Urine has been observed to be as high coloured as *Porter's Beer*.

I am informed, by the Parriets and Cowlceeches, that an Horse or a Cow will bear to have near two Gallons of Blood taken away without fainting. One Cow, I have seen, within about a Month or six Weeks of her Calving-Time, was taken with the Running at the Nose, and Shortness of Breath; the Owner of her immediately took away out of the Neck five Quarts of Blood by Measure, and gave her a warm Mash of Malt once in six or eight Hours; Next Day he cut her Tail, and let her bleed two Hours; the Day after he took away two Quarts from under the Tongue, and so continued bleeding her, at fourteen or fifteen Hours Distance, for seven times. She did not purge at all; her Urine was as high-colour'd as Coffee at first, but grew paler and paler every time of bleeding: She soon recover'd, now eats heartily, looks brisk, and has not slunk her Calf.

The Concern the Cow keepers are under for the Loss of their Substance, the various Methods offer'd to them, and their Want of Judgment either to chuse the most rational, or their Want of Accuracy in making Experiments, and following Directions, is quite discouraging, and is the Reason why none of them have pursued any Regimen so steadily as to give one an Opportunity of making Conclusions from it: Indeed several own to me, they are quite bewilderd, not knowing which way to turn themselves, or whose Advice to follow, what one says being quite contrary to the Directions given by another. Some to whom I have given my Directions have bled once, have given the Purge once; but have

have not given the oily Drench, or have given this once, and have not repeated it; others have given the chalky Drench once, and not repeated it, and have not followed the other Parts of my Instructions; so that I am sorry to find that I can have no satisfactory Experiments made: Yet, as the State of the Disease seems so evidently to be a Peripneumony, or Inflammation of the Lungs, Windpipe, and Nostrils, attended with a Redundance of Gall, I cannot forbear urging to the Public the following Method.

“ Give to all Cows in general, while well, half
 “ an Ounce or an Ounce (according to the Size
 “ of the Cow) of *Crocus Metallorum*. As soon
 “ as a Cow falls off her Meat, give her another
 “ Dose of *Crocus Metallorum*; and give her
 “ warm Mashs of Malt, Bran, &c. When she
 “ runs at the Nose, lay a Bag of Malt-Meal,
 “ wetted with boiling Water, upon her Fore-
 “ head and Nose, tying it to her Horns, Morn-
 “ ing and Evening; pour warm Vinegar and
 “ Salt into the Nostrils: If a short Cough, or
 “ Difficulty of Breathing, comes on, bleed her
 “ one Quart twice a Day, for three or four
 “ Days, and every six Hours give the oily
 “ Drench: If a Purging comes on, give another
 “ Dose of the *Crocus Metallorum*; if it conti-
 “ nue, give the chalky Drench every six Hours,
 “ and if it does not abate in twenty-four
 “ Hours, inject the same Mixture by way of
 “ Glyster; and if the husky Cough continues
 “ with the Purging, give the oily Drench one
 “ three Hours, and, the chalky Drench the next
 “ three Hours.”

Most of the Cows which have recovered from this Distemper recover their Milk again, as their Appetites mend; but they are observed to have scabby Eruptions come out in their Groins and *Axilla*, which itch much; for a Cow will stand still, hold out her Leg, and shew Signs of great Pleasure, when a Man scratches these Pustules or Sores for her.

I am informed, that some Cow leeches have given *Coloquintida* and Salt of Tartar, each one Ounce, in a Quart of warm Ale; but I imagine it must be too griping a Purge, and improper where the Guts are inflamed. Indeed I have not heard of any Cows recovering which took it.

As for the Cause of this Distemper, I am still at a Loss; I think it cannot be owing to the Food, because the Cows which had it first in *Essex* eat only Grass, Turneps, and Hay or Straw; the Cows about *London* eat, some, Grass; all, Grains and Hay, some, little or no Grass, but live chiefly on Grains, Turneps, Off falls from the Garden-grounds, and Hay.

I am in Doubt as to the Air; the Spring and Summer were very wet, and the Ground very damp, the Autumn was very dry and cold, the Beginning of Winter very damp and cold. The Cows in *Essex* had the Distemper in Summer; it first began about *London* in Autumn: It has spread itself equally among Cows which have lain in the Fields a nights, and those which stood in Stables or Sheds: It spread itself in *Essex*, at first into such Farms where they bought in strange Calves, or lean Cows, at Market, which they did not know where they came from; the most probably from the Hundreds where the Distemper first broke out; but how it got thither, whether

whether by importing any Cattle from *Flanders*, I know not; for surely there is too wide a Tract of Sea for any infectious *Miasmata* to be wafted over to that Part of the Country by the Winds! This is certain, the *Viscera* concerned in Respiration are the Parts chiefly affected. Its Spreading here in *England* has been progressive; and therefore one may reasonably think it is not constitutionary in the Air, for then it ought to be universal every-where; but that it is contagious, and propagated by infected Cows being mixed with well Cows: Therefore the not buying in Calves, or strange Beasts, but every Farmer keeping his Herd by itself, must be a great means of preventing the Propagation of it: And housing the Cows a-nights may be a proper Preservative against it.

XIV. *Extract of a Letter from Mr. Arderon to Mr. Baker, F.R.S. giving an Account of the Weaver's Alarm, vulgò LARUM.*

Read Dec. 12. 1745. **N**OTHING is more true, than that Necessity is the Mother of Invention; among the many Instances of which, the useful Contrivance I-am going to describe may serve as one remarkable Example.

This little *Apparatus* goes commonly by the Name of the Weaver's *Larum*, from its being chiefly or originally made use of by Persons employed in that Trade, who have frequently Occasion to get up very early to their Work: And, as I am informed,

Norwich may boast of its first Appearance there, though I am unable to learn the Inventor's Name. However, the Simplicity of the Thing itself, and the singular Service it may be of to Multitudes of People, renders it (I believe you'll think) not undeserving Notice.

The Materials necessary to compose this little Time-Piece or Monitor, are nothing more than a small Candle, of fourteen or sixteen Inches in Length, a Piece of Thread or Packthread, a graduated Board, and a common Stone, or any other ponderous Body : But the Drawing added hereto (TAB. I. *Fig. 3.*) will fully explain my Meaning.

A Represents a Board, which hangs commonly against a Wall, divided and figured according to the Size of the Candle made use of *.

B, A little Shelf to place the Candle on.

CC, A Thread or Packthread, tied fast at *D*, and hanging over a Pulley at *E*, whereto a Weight is hung at *F*.

By sliding the Spring of the Candlestick *G*, up or down, as Occasion requires, the Flame of the Candle is raised as many Hours above the Thread as the Person that adjusts it designs to lie before he is called up. At the desired Hour the Candle burns the Thread in two, the Weight falls, and, by its Noise, seldom fails to wake the Person.

But

* For want of such a Board a common Ruler is frequently used, to measure the Number of Hours between the Flame of the Candle and

But if the Man who makes use of this Contrivance happens to be of a more than commonly sleepy Disposition, in such Case another Thread is tied to that Part of the Line CC which is next the Pulley, and its other End is twisted round the Thumb or Wrist of the sleepy Person, whereby, when the Candle burns the Line, and the Weight falls, he receives such a sudden Pull as can hardly fail to wake him, as the Drawing will easily explain.

If the Line for a few Inches on each Side the Candle be Wire, with a short Thread only just in the Middle where the Candle is placed, there can be no Danger of doing Mischief by the Fire's running along the Line.

And thus may the poorest Mechanic provide himself with an useful Servant at a very small Expence.

Dec. 10. 1745.

XV. An Account of some human Bones incruſted with Stone, now in the Villa Ludoviſia at Rome: communicated to the Royal Society by the Preſident, with a Drawing of the ſame.

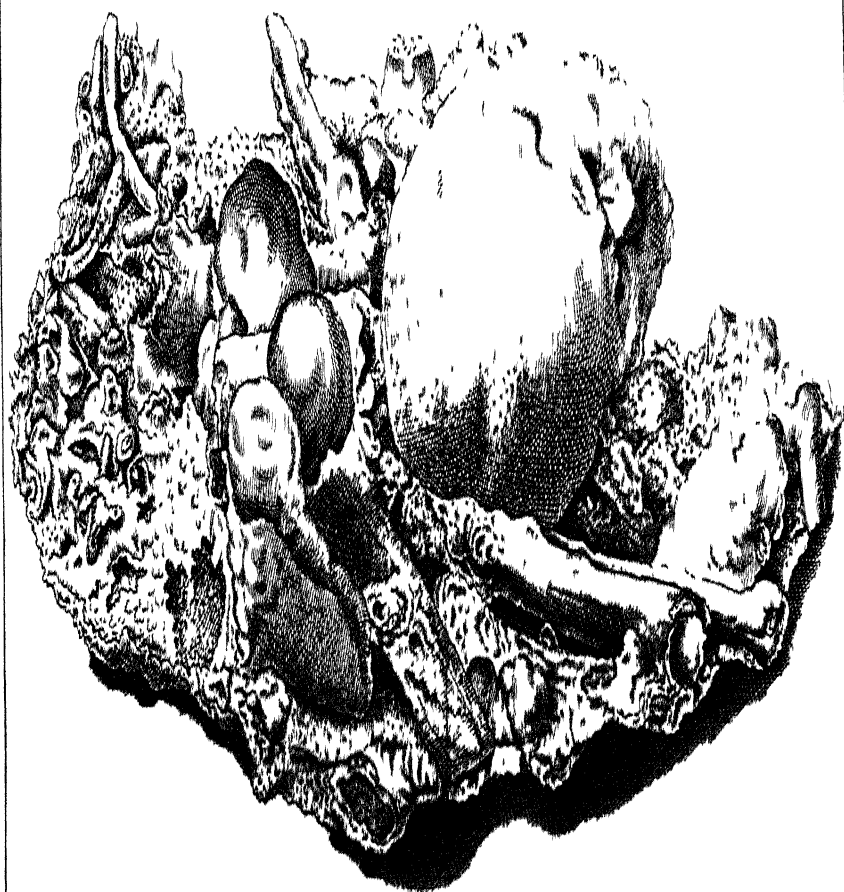
Read Dec. 12. **S**OMETHING like the Body of a petrified Man being mentioned by ſeveral Authors, as preſerved in the *Villa Ludoviſia* at *Rome*, and the ſame having been lately referred to in a Diſcourſe read before this *Society*; I thought, that a Drawing of that Curioſity, which I procured at *Rome* ſome Years ſince, might, poſſibly deſerve the

the Notice of the Gentlemen here present, especially, as it will appear thereby, that the several Accounts hitherto given of it are not very accurate, or, at the best, convey but a very imperfect Idea of the Truth.

The following Passage occurs, in the Journal-Book of the Society, for the 17th Day of April 1689: "Mr. Henshaw related, that he had seen, in the *Villa Ludovica* at Rome, the Body of a Man incrusted with a sort of a white Marble or Alabaster Case, supposed to have been a Man frozen in the *Alps*, and after, in long Proceets of Time, this Incrustation to have grown upon him; and that one of his Arms was broken off, purposely to shew, that it was no Imposition."

Mr. *Richard Lassels*, in his Travels to *Italy*, printed at *Paris* 1670. pag. 180. tells us, that in the lesser *Casina*, belonging to the *Ludovisian Villa*, he saw, "in a great square Box lined with Velvet, the Body of a petrified Man, that is, a Man turned into Stone; one Piece of the Leg (broken off to assure an Embassador doubting of the Verity of the Thing) shewed plainly both the Bone and the Stone crusted over it. The Head and the other Parts lie jumbled up together in the Box."

Father *Athanasius Kircher* says, in his *Mundus Subterraneus*, l. viii. chap. 2. "Spectatur et hic Romæ in horti Ludovisiani palatio, corpus humanum totum in lapide conversum, ossibus adhuc integris, at lapides carice obductis." And in the following Page he gives an imperfect Sketch of the same thing, under the Title of "Sectum humani corporis in lapide conversum, in palatio Pinciano præcipue." This Sketch, however imperfect, gives



a truer Idea than either his or Mr. *Laffels's* Words seem to convey, as there is indeed nothing like the Body of a Man, but only a Cluster of disjointed Bones cemented together by the same Matter that incrusts them over. Mr. *Misson* in his Travels has more truly described them, when he says, that “ in the same Room they shew a small Heap of Bones, said to be the Skeleton of a petrified Man; which is a Mistake, for the Bones themselves are not petrified, but there has gather’d about them a sort of candied Crust, or stony Incrustation, which has made them pass for being of real Stone.” Mr. *Wright* also, in his late Observations made in travelling through *Italy*, &c. has taken notice, that in the *Villa Ludovisia* “ they shew’d some Bones of a human Body all crusted over with a petrified Substance.”

When I was at *Rome* in the Year 1734, I myself saw this Curiosity, which is still preserved in the same *Casina* of the *Ludovisian* Gardens; and in the very square Box lined with Velvet, that is mentioned by Mr. *Laffels*, and represented by Father *Kircher*: and as I had before heard it much spoken of, and had conceived an Idea of it very different from the Truth, I was willing both to preserve a true Notion of it myself, and to be able to give such a one to others. I therefore employed an ingenious young Painter to make as exact a Drawing of it as he could; and I afterwards very carefully compared his Drawing with the Original, which is the same I have here to produce, hoping that the Sight of it will not be unacceptable to the Company. The stony Substance that joins the Bones together is of a whitish Colour, and the same as that which incrusts the Bones themselves: small
Fractures

Fractions in several Places discover the natural Bones; and the Size of the whole Mass may be judged of, by considering the Skull, which is of the common Dimensions, as a Scale to the other Parts. *See* TAB. II.

F I N I S.

To the Book-binder.

The *Crounean* Lectures on Muscular Motion, for the Years 1744 and 1745. are to follow this Page, and stand before the *Index*.

THE *Crounian* LECTURES.

A N

I N D E X

T O T H E

Forty-Third VOLUME

. O F T H E

Philosophical Transactions,

For the YEARS 1744, and 1745.

N. B. By a Mistake of the Printer, the Pages are number'd from 1. to 102. twice over; viz. in n. 172. which begins this XLIII. Volume, and in n. 173. and Part of 174. but no Mistake can arise from the *Index*, because the n. is always joined to the Page. And as for the SUPPLEMENT, the Letter s. stands before each Page of it, and *pr.* denotes the Preface to it.

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THE
CROUNIAN Lectures
ON
MUSCULAR MOTION.

For the Years MDCCXLIV and MDCCXLV.

Read before the
ROYAL SOCIETY:

By JAMES PARSONS, M. D. and *Fellow*
of the ROYAL SOCIETY.

Being a SUPPLEMENT to the *Philosophical*
Transactions for those Years.

*Mundi Pars est Aer, & quidem necessaria: hic est enim qui Cælum
Terramque connectit.*

SENEC. *Nat. Qu.* l. 2. c. 4.

*Animantes autem adspiratione aeris sustinentur. Ipse enim Aer
nobiscum videt, nobiscum audit, nobiscum sonat; nihil enim sine eo
fieri potest.*

CIC. *de Nat. Deor.* l. 2. c. 33.

L O N D O N:

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TO
MARTIN FOLKES, Esq;
PRESIDENT,
AND TO THE
COUNCIL and FELLOWS,
OF THE
ROYAL SOCIETY,
THESE LECTURES
ON
MUSCULAR MOTION

Are humbly Dedicated by

Their Most Obedient,

And Most Humble Servant,

JAMES PARSONS.

A

P R E F A C E.

I.

AS the following Sheets were composed rather by Injunction and Duty than Choice, it is hoped their Imperfections may claim some Indulgence from the Reader; since such Freedom can scarce be exhibited in the Prosecution of a Subject so difficult and confined, as might be expected, if it were more agreeable to the Author, or less limited and particular in its Nature.

However, I flatter myself that something New will be found in what I offer as my own; which, perhaps, is capable of being further improved hereafter, towards the Explanation of the Manner in which Muscular Motion is performed.

II.

The Air is a principal Agent of Motion thro' all Nature, and may be called the Spring of all moveable Bodies. No Animal can move on the Earth, in the Atmosphere, or Waters, without its kindly Assistance; nor can there be any Growth of either Animal or Vegetable Bodies, unless promoted by this most useful Element: In short the Phænomena in Nature are numberless, that necessarily require its Aid. In this Light, I find it highly concerned in the Actions of Animals; which, I think, will clearly appear by the Use I make of it in accounting for those of their Muscles: And I find, that without supposing it so, no probable Conjectures can be produced towards accounting for that Motion, which a Review of the several Opinions of the Authors mentioned in my first Lecture will sufficiently testify.

III.

What the Soul is, or in what Manner she makes her Impulse on those Parts of Animals
that

that are the immediate Instruments of Motion, we dare not attempt to guess; these being wrapp'd up among those Secrets only known to HIM that order'd all Things: But, as the Bodies of Animals are mechanical, and therefore naturally fall within the Sphere of our Understanding, we may make some Attempts towards explaining the several Phænomena that belong to it; and therefore we can only consider how its Organs are actuated, and not what is the Cause of their Motion; and must take it for granted, that the Soul makes her Impulse on the Organs, and then endeavour to shew the Nature of the several Consequences of that Impulse, as far as it relates to the Motion of the Muscles.

IV.

And this is carried on, as the Reader will find, upon a Plan and Foundation not merely conjectural, as is the Case of most Writers on this Subject, but (1.) upon the Knowledge of the Structure of a muscular Fibre; (2.) upon a due Consideration

sideration of the Use of the Interstitial Air, and its Counter-action with that contain'd in the nervous and muscular System; and (3) upon my Observation of the Circulation of the Blood, in the minute Vessels of several Animals, and their parallel Direction to other Fibres: All which Particulars are my own Discoveries; and, being duly connected, seem to me the most likely, that have hitherto appeared, to give some Light into the Nature of Muscular Motion.

V.

These Things naturally lead me to consider the animal Body as a Machine consisting of Two complete Sets or Systems of Organs, each perfect in itself, and each containing a Fluid peculiar to itself, and different from the other. The first of these is that of the Laëteals, Blood-vessels, and Lymphatics, continued to each other, and containing its proper Fluid, the Mass of Blood, and its derivative Juices; and the other is the Nervous and Muscular System,
which

which are also continued to each other, and having their proper Fluid the elastic Aura or Air; which are more fully explain'd and connected in my second Lecture, together with an Account of Two Kinds of Æquilibria proper to Muscles, deducible from these Systems.

VI.

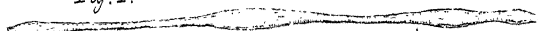
*I have endeavoured, as much as possible, to be brief in my Explanation throughout the Whole; avoiding every Digression that might in the least interrupt the Chain of my Reasoning: And as I have made some Objections to the several Opinions in my first Lecture, which I think they seem'd liable to (in order the better to arrive at the Truth, yet, with all possible Regard to their Characters and Learning), the World is welcome to use me with the same Freedom; and indeed I shall be always glad, if any Improvement arises from such Objections to my Sentiment as the Judicious may justly make;
desiring*

desiring no more at their Hands, than the same Candour, in their Inquiry into mine, that I have shew'd in my Examination of the Opinions of others.

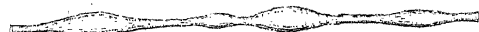
THE

TAB. I.

Fig. 1.



1



2

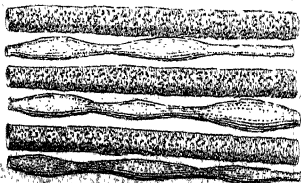


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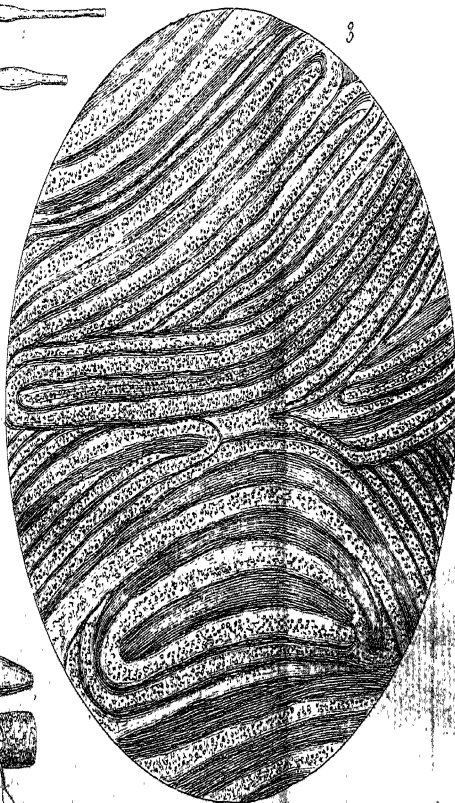
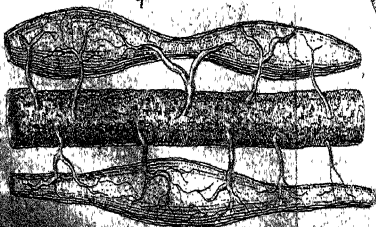


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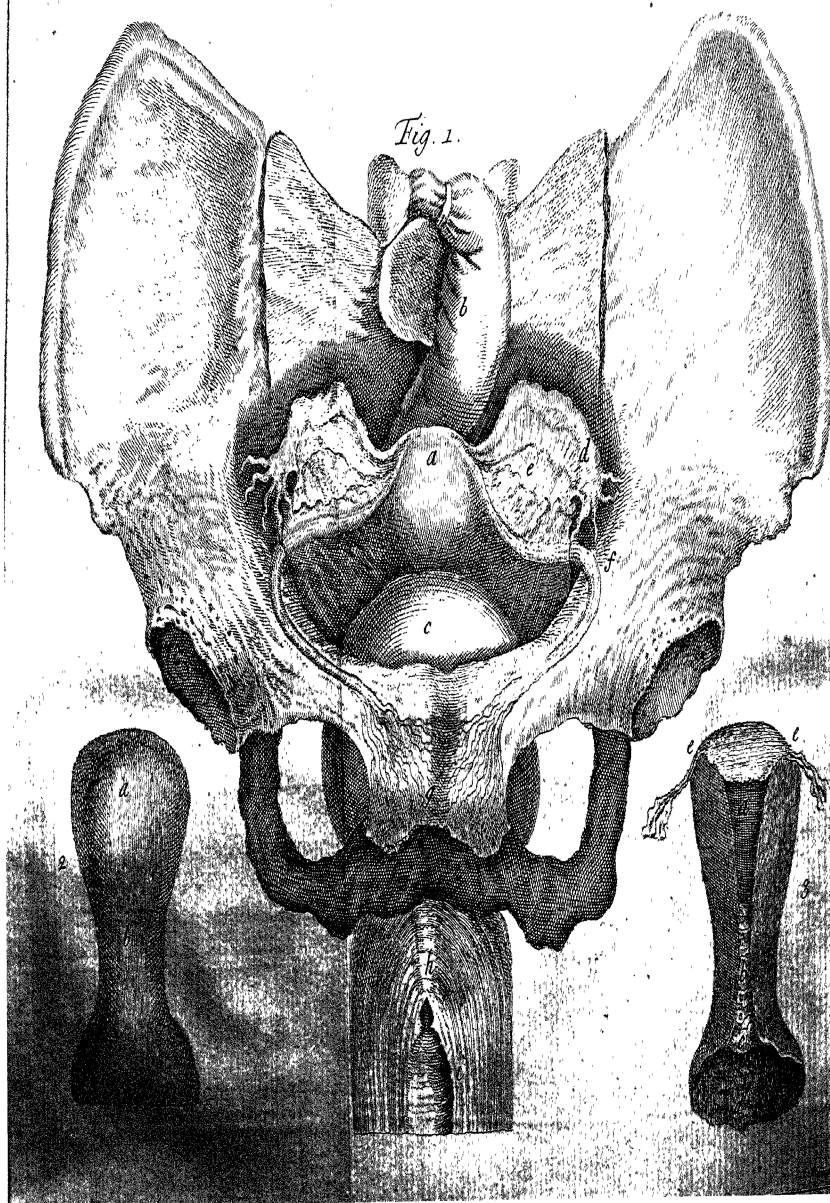
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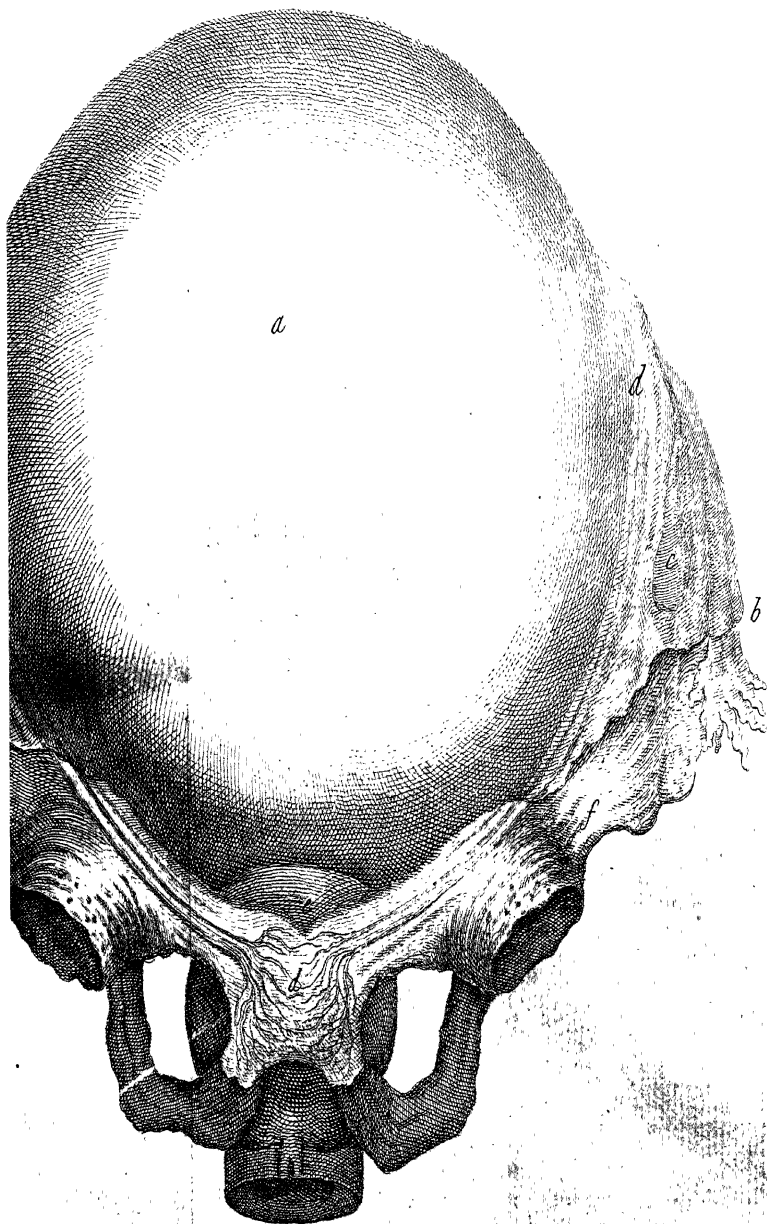
TAB. II.

Fig. 1.



TAB III





THE
CROUNIAN Lectures
ON
MUSCULAR MOTION.

LECTURE I.

SECT. I.

Read in January
1743-4

THE proper Motion of the Muscular Fibres of Animals, and the Manner of its being communicated to, and continued in them, is a Subject so abstruse, that it is with the greatest Diffidence imaginable I have attempted its Explanation. It is the last Subject I would offer at, of all that the boundless Scope of Nature affords us. And, indeed, no other Motive should have engaged me in it, than to comply with the Command of the worthy *President* and Council of the *Royal Society*, for whom I bear the utmost Esteem.

B

II.

II.

In this Essay I have nothing in my View but the Consideration of a *muscular Fibre*, and that of a *nervous* one, with the Manner of Muscular Motion's being performed; having purposely neglected to touch upon the Nature of Sensation, or indeed any Calculations of the Force or Powers of Muscles; because they are already well treated of by several ingenious Authors; and are capable of being handled upon Rules of some Certainty; being a Part of the Subject very different from what I take to be the Purpose of the *Crownian Lectures*.

III.

Since, then, those worthy Gentlemen have done me the Honour of appointing me to continue these Lectures on *Muscular Motion* for the present Year, according to the Will of the late Lady *Sadler*, I shall endeavour, to the best of my Power, to gratify them; yet confess myself very unequal to the Task; especially, as several of the most learned and ingenious Physicians of all Ages have hitherto undertaken it with so little Success. However, in order to render what I shall advance upon the Subject the more clear and satisfactory to the *Society*, some of whom may not have made this Part of Philosophy their Study, I presume it will not be disagreeable to premise the following brief History of the most remarkable among the Opinions that have gone before us, by way of Introduction; which will also serve to facilitate the Consideration of this Subject, to whosoever shall
be

be appointed hereafter to undertake the same Task, by bringing the several Opinions together in a small Compass before him.

IV.

Most Authors agree, That a Fluid, commonly call'd *Animal Spirits*, flows from the *Brain*, by the *Nerves* to the *Muscles*, in order to move them; but are at a Loss to know how it is performed; and also, by what means those Spirits are sent, so swiftly, into this or that Muscle to be moved.

V.

Some endeavoured to explain it, by supposing certain *Valves* placed in the Cavities of the Nerves, (where they are divided into Branches *, to go to different Muscles) in order to stop the Reflux of the Spirits, and cause them, upon being brought back from one Muscle, to be determin'd to the other, from the Valve.

VI.

Others, not well satisfy'd with this Scheme, imagined a double Tube, passing from one Muscle to the other, so placed, as that the Orifice of one, in its Contraction, (being furnished with a particular Valve) might be opened, and the Spirits immediately flow through it, from the Muscle to be relaxed into that

B 2

to

* *Cartes. lib. de Homine*, and several of his Followers, were for placing Valves only in the Divisions of Nerves; whereas *Regius, Philosoph. Nat. lib. 4. cap. 16.* thought them to exist elsewhere in the Nerves.

to be contracted; whilst, at the same Instant, the Valve of the latter is shut, in order to hinder their Flowing-out again, that the Muscle may be swell'd. By this Swelling the Situation of the Parts being altered, the Valve opens again, (the other Valve being now shut) and the Spirits flow freely back again to the Muscle to be contracted.

VII.

Cartesius's * Opinion was not very different from this: He supposes " Several Openings in each Muscle, through which the Spirits may pass from one " into the other; which are so disposed, that, when " the Spirits, which come from the Brain towards " one Muscle, have a little more Force than those " which go towards another, they open all the Orifices through which the Spirits of the other Muscle " can pass into this; and, at the same time, shut up " all those by which the Spirits of this may pass into " the other; whereby all the Spirits, contained before in both Muscles, swiftly pass into one of them, " and so swell and contract it, while the other remains relax'd and extended."

VIII.

These Tubes, Valves, and Openings, are merely conjectural; having never been found by any Anatomists, and being only Children of the Imagination of some Philosophers, produced to serve their uncertain Hypotheses. Nor, indeed, would it be a difficult Matter

Matter to shew, that neither of these Systems can account for Muscular Motion; and that such a Structure would produce Confusion, instead of that most regular and uniform Process we daily see, in every Action of Life, if our intended Brevity would permit it; which must be obvious to every one in the least acquainted with the Structure and Situation of Muscles. Let us, however, consider other more particular Opinions in their Turns.

IX,

The first I shall take notice of is the memorable Doctor *Croune*; whose Care for propagating the Knowledge of this Part of Physiology, is no less laudable than evident, in his Foundation of these Lectures. He shew'd a good Example in his own Attempt towards an Explanation of Muscular Motion, in a Treatise intituled, *De Ratione Motus Musculorum**: Wherein, after a short Recapitulation of some Opinions before him, particularly of those of *Cartesius*,
Regius,

* This Treatise was published at *London* in 1664. in 4to. and at *Amsterdam* in 1667, in 12mo. and its being published without his Name, occasion'd a gross Mistake in the Publisher of Dr. *Willis's* Works at *Geneva*; who printed it among that Author's Tracts, notwithstanding there is a particular Tract in the very same Volume, known to be *Willis's* own, intituled, *De Motu Musculari Medico-Physica*. The Editor's Apology for so doing is as follows: ' *Tractatum de ratione motus musculorum, (et si authoris anonymi) anatomie cerebri, nervorumque descriptioni ab excellentissimo viro D. Thomæ Willis, M. D. celeberrimo, institutæ, (ut alias, tum a materia affinitate, tum ab operis præstantia petitis, missas faciam) quod in prioribus editionibus magnæ cum docturæ applausu, & sine ulla, quod sciam, clarissimi D. Willis quærela, appositus reperitur.*' See the *Geneva* Edition in 4to, an. 1680.

Regius, and *Dr. Scarborough*, he endeavours to found his Reasoning upon Mechanical Laws, in accounting for Muscular Motion; and lays it down as a principal Maxim, That the Motion of every Muscle is begun by a certain spirituous Liquor passing from the Nerves; but is accomplished, or finished by two other necessary Causes which succeed it. He despairs that any one can ever arrive at the Knowledge of the Manner in which the Soul acts upon the Body, and therefore avoids troubling himself about it; but produces many Reasons to shew, that whatever it is that gives Motion to the Muscles, must necessarily pass by the Nerves. This leads him to inquire into the Structure of a Nerve; which, he says, "Is composed of a certain medullary Substance full of Juice, with a double Membrane which involves that Substance; and also an infinite Number of little Cords within these Membranes and medullary Substance, extended from their Beginning to the very extreme Capillaments", which are dispersed and inserted into the Parts of the Muscle.

X.

But *Dr. Croune* refers to it himself as his own, in a Paper, p 25, intitled, *An Hypothesis of the Structure of a Muscle, and the Reason of its Contraction*. [Read in the *Surgeon's Theatre*, anno 1694, 1695]. This (says Professor *Ward*) is the Substance, or Heads only, of the Doctor's Discourses upon that Subject, published by Mr. *Hook*, in his *Philosophical Collections*, Num. 11, Sect. 8. P. 22.; which, being afterwards translated into Latin, was inserted in the *Acta Eruditorum*, anno 1682, p. 194, with the Title *De Motu Musculorum*. See that learned Author's *Letter of the Professors of Gresham-College*, p. 323.

X.

In speaking of these Spirits, he says, That the alimentary Juices abound with very subtil active Particles; which, by their frequent Circulation with the Blood, are gradually freed from the terrestrial Parts, wherein they were confined. These are in great Plenty in the arterial Blood; which, being carried through the Arteries of the Brain, deposits in its medullary Substance, by a slow Kind of Distillation, a Fluid, which our Author calls a *Mercurial Liquor*, that is- (says he) exquisitely impregnated with a *volatile Salt* and *Sulphur*, which flows from thence into all the Nerves of the Body, passing every way through them slowly, and at length falling into the Veins by a gentle Circulation, till they arrive again at the Heart: And that, by these spirituous Liquors, all the Parts of the animal Body grow very turgid, and are kept in continual Agitation, assisted by the Circulation, and the *Calor nativus*. And this Agitation is what he calls the *very Life*.

XI.

And although this Author allows the Nerves to abound thus with this rich *rectify'd Juice*, yet he denies that they are regularly tubular, as Authors affirm, but only as they are defined above; and also that any Kind of Cavity can exist in a Muscle; and, consequently, that there can be no *Inflation* of its Parts: But is of Opinion, that in every Muscle there are three Kinds of Spirits; one peculiar to the *Tendons* and their Fibres, another to the *Muscular Flesh*, and another which comes to the Muscle by the *Nerves*.

XII.

These spirituous Liquors, (says he) together with the Membranes of the Body, are the Instruments of Sensation also. For he concludes, that all the sensible Membranes of every Part of the Body arise from the *Meninges* of the Brain; and that they are all kept in a Kind of Tension, by these spirituous Liquors passing constantly thro' them. In this State of Tension or Tone, he thinks that they may be compared to a Glass, or Bell *, whose Parts have a vibrating Motion communicated all over them, by being touch'd in One Part. Thus, (says he) by the Intermediation of the Membrane of the Nerve that belongs to any particular Organ of Sense, or by means of the one common Membrane which involves the whole Body, every Object of Sense is carried, as much as can be, by right Lines, to the Brain; wherein the various and distinct Motions of Objects are perceiv'd by the Soul. Hence this ingenious Author would endeavour to shew how, in a *Paralysis*, Sensation should remain when Motion is lost, and the contrary: For that if that Tone of the Membranes should at any time be totally, or in Part, destroy'd, by either Change of Situation of their Particles, or by the Access of too much Moisture, or any Division of their Continuity

* There seems but little Analogy between such vibrating Bodies and Membranes. It is, indeed, the Property of a Bell or Glass to vibrate and sound, when struck, provided it be pendulous, or otherwise free: But, if any Part be touch'd, its Vibration and Sound are impeded. How much less is a *soft* Membrane, in the Body, capable of Vibration; since it is in a close Contact with other

nuity by an Accident, that then, indeed, that Vibration or Undulation of Particles, which causes Sensation, would be interrupted; like a cracked Bell or Glass, which, instead of its agreeable sharp Sound, exhibits a jarring disagreeable Noise.

XIII.

Our Author defines a Muscle, as consisting of an infinite Number of *tendinous Fibres* like Cords; which are so blended together at the Extremities, as to resemble a thick Cord composed of many others; but that within the Body of the Muscle those Fibres are at some Distance from each other, and the Spaces between them filled up with *Flesh*; which, with Membranes, Blood-vessels, Nerves, and innumerable Lymphæducts, constitutes the intire Muscle. This *Flesh*, in the Spaces between the Fibres, he says, is nothing else than that Portion of Blood flowing thro' these Interstices; which, being condensed * by the Coldness of the Fibres, is detain'd between them, and constitutes the Muscular *Flesh*.

XIV.

He has given an ingenious Scheme for explaining the Manner in which a Muscle is moved, after having laid down the above *Præludia*; the Sum of which is, That a certain Power is determined from the Brain,
by

* It is to be fear'd, if Blood could be evasired, the least condensed Particle would be sufficient to form, by degrees, Abscesses, or some other Mischief.

by the Will, with these Animal Spirits, through the Nerve to the Muscle to be moved ; which causes the first Tumeſcence of the Muscle : And that the Soul has an *Imperium* *, through the whole Maſs of Blood alſo ; as is manifeſt in the various Paſſions of Anger, Joy, Love, Baſhfulneſs, &c. whereby ſhe is capable of determining the Blood to any Part in a greater Quantity than ordinary, and, conſequently, to the Muscle to be moved : For, ſays he, it is not abſurd to imagine, that the ſame *Idea*, which excites the Will to move a Muscle, and the Spirits in the Nerve to perform it, in like manner is capable, at the ſame Inſtant, of determining Spirits to the Heart, by the Nerve

* It will appear in the next Lecture, that the *Soul* can have no *Imperium* over the Blood, and only preſides over the voluntary Action, of the Body ; for the Heart drives the Blood indifferently to all Parts of the Body, for its Welfare, by a propelling Force, which is involuntary : Nor can the Motion of the Heart be accelerated immediately by the Will, but only by ſome particular Act of the Body before ; ſuch as an Increate of Exerciſe by Running, &c. And as to thoſe Paſſions of Anger, Baſhfulneſs, Joy, &c. they firſt occaſion quick and irregular Reſpiration : This cauſes a quicker Motion of the Heart, which warms and rarefies the Blood more, whereby it is driven with greater Velocity to all Parts, and among them to the Cheeks. Thus Bluſhing happens, not becauſe the Soul determined more Blood than ordinary to them, but becauſe its *Momentum* is increaſed involuntarily by the Shock or Surprize previous to it : For the whole Surface of the Body is affected in the ſame manner, tho' moſt apparent in the Cheeks. Hence no Increate of Blood can be determined to one Part more than another ; unleſs ſome Impediment happens to its free Circulation in one Part, or the Reſiſtance becomes leſs in another ; which we ſhall hereafter ſhew. But beſides, one would be apt to think, if the *Soul* had an *Imperium* over the Blood, ſhe might as well prevent the Effects of Poiſon, or any other Malignity in it, and correct the Maſs ; as immediately determine it to more than ordinary Quantity to any particular Part of the Body.

Nerve which is inserted and dispersed thro' its Auricles, and causing it to propel Blood more copiously to the Muscle.

XV.

The Use he makes of this is, That a third concurring Cause of Motion in a Muscle should be brought in, in order to render it more complete; and that is, A Fermentation produced by the Animal Spirits of the Nerves, and what he calls, the Spirits of the Blood; which he compares to that of any two chymical Liquors mixing together: And that when this Agitation is begun in the Membranes of the Muscles, the Fluids will be driven, by Their *Nisus*, in right Lines towards the Extremities of the Muscle; but that, finding the Spaces much narrower in them than in the Belly of the Muscle, they are driven back to the Middle into the Muscular Flesh, where the Pores are larger, and more lax; which makes the Muscle swell, by the Particles endeavouring to recede from each other, and occupy a larger Space; as, says he, happens in all Fermentations: From hence, as the Spaces are made larger in this Muscle, there is Room made for the Access of more Blood from the Artery in the moving Muscle. Thus Muscular Motion is performed (according to our ingenious Author) by three conjunct Causes; *viz.* *Animal Spirits* flowing to the Muscle, *Arterial Blood* determined in greater Quantity than ordinary, and a *Fermentation* * raised by

* We shall find this learned Author's *Fermentation*, as it serves to account for Muscular Motion, seiz'd on by most of those that followed him, without giving him Thanks for it.

by their Admixtion, by which the Muscle is swell'd and shorten'd.

XVI.

Our Author has not only manifested (in his Treatise) much Learning, but also a penetrating *Genius*, in many curious Remarks and Observations dispersed through the Whole; and merits the Regard of the learned World no less than some Authors we shall mention by-and-by; who, by pluming themselves with *his* Feathers, had monopolized much Esteem and Attention from Mankind, by the Exhibition of this System, with very little Addition; and may indeed be justly said to have led several of them, by his Hints, into their most favourite Notions concerning *Muscular Motion*.

XVII.

Steno. *Steno* *, another ingenious Author, wrote his *Conjectures* on this Subject much about the same Time with Dr. *Croune*; but had a very different Notion from what we have just mention'd concerning the latter; and which, for its Particularity, shall have a Place here; especially as it will appear hereafter, that he laid the Foundation, upon which *Borelli* rais'd his Hypothesis, as to what regards the Structure of a Muscular Fibre; besides what Assistance he has had from Dr. *Croune* on other Accounts.

XVIII.

XVIII.

This Author, after having expatiated a good deal upon the different Structures of the Muscles, and made several learned Observations thereon, makes his general Conclusion to the following Purpose:

1. That Arteries, Veins, Nerves, Fibres, and Membranes are the constituent Parts of a Muscle: And that, though some Authors have pretended to have found Lymphatics, he never could find any.

2. That there is no Muscle whose Fibres do not terminate in a Tendon; which are collected either into firm Tendons at one or both Extremities, or are diffus'd into Membranes, or degenerate into an officious Rigidity.

3. That those very Fibres (which, being closely connected together, compose the Tendon), when join'd more loosely, constitute the Flesh or carnosus Substance; and that therefore the Flesh is not a particular *Parenchyma*. In which Sense a Tendon is a continued Body from the Beginning of a Muscle to the End.

4. That there is scarce any Fibre in a Muscle which constitutes a right Line; but every one is divided into three Lines at least, which comprehend two alternate Angles. Nor are these three always right Lines; but often the Flesh, as in the *Sphincters*, and sometimes the Tendons, form curved Lines.

5. These three Lines of every Fibre in the same Muscle, are not always of the same Length; altho' there is scarce any Difference of Extension among them when acting all together: That these Fibres are in the same Plane; and that, in this angular Order, they form *oblique-angular Parallelograms*, or *Rhomboidal Figures*;

gures; whose two opposite Parallel, in the Line where they comprehend acute Angles with the other Parallels, are stretched to the opposite Parts without the Angles. Here he makes a Remark, That in every Tendon, tho' never so slender, there are as many Filaments as there are fleshy Fibres in the Belly of the Muscle; and the interior Filaments are shorter than the exterior.

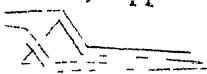

6. His sixth Conclusion is taken up in the Division of a Muscle into Extremities and its Middle, with some Animadversions on that Head.

7. He says also, That a Membrane not only goes round each Muscle with a transverse Direction of its Fibres, but that it also insinuates itself in the same manner between every muscular Fibre.

XIX.

When he comes to speak of the Action of a Muscle, he confesses ingenuously, that he cannot attempt explaining the Cause or Manner in which it is performed; yet makes a Comparison towards an Explanation, which he thinks not improper, but wherein, I must say, I cannot find any Satisfaction. Imagine, says he, a Machine for driving Piles into the Ground drawn up by several Men, each having his particular Cord which is fasten'd to the main Rope: The *Cords* imitate the Tendons; the *Weight* fixed to the Cords, the moveable Part; and the *Men*, the fleshy Fibres: For, says he, as Men (being render'd shorter while they pull their Cords) move the Weight, so the fleshy Fibres, being contracted, by drawing the Tendons, pull the moveable Part. There is no more understood by this Comparison, than that the Contraction
of

of the fleshy Fibres causes the Tendons to come nearer to each other, which every-body has said; but the grand Question is, How they are contracted? And as to this particular Structure of the Fibres, I believe none but the Author himself ever fancied he saw them so. For, supposing this angular Form in every

one  to be the real Structure, the Contraction would reduce it to this Form 

But the Cause and Manner how this is brought about upon this Plan is inexplicable; especially since much Force is required to draw the Weight or moveable Part. But enough of this Author: Let us now proceed to give a short Sketch of the Opinion of a much more famous Author, the great Dr. *Willis*.

XX.

He commends the foregoing Author very much for his ingenious Conjecture about the Structure of the Muscular Fibres; and gives a Detail of what that Author has said concerning them, with an Air of Approbation; but attempts accounting for the Cause and Manner of the Performance of Muscular Motion, of which the following is the Sum: *Willis.*

XXI.

Animal Spirits are carried from the *Encephalon*, by the Nerves, to every Muscle; and are received by the membranous *Fibrillæ*, and by these forwarded into the tendinous Fibres; there to be reserv'd as in a proper Receptacle or Store-house. These Spirits, as they are of a most active and elastic Nature, as often

as it is necessary, expand themselves, and fly swiftly into the fleshy Fibres; and, having made their *Impetus*, recede into the Tendons, by turns. But, while these Animal Spirits pass into the fleshy Fibres, at the proper Instinct for performing the Motion, they meet with very active Particles of another Kind supplied by the Blood, which ferment together; so that, from their Strite and Agitation, the fleshy Fibres, which were before lax and porous, are fill'd up, and are forced into Contractions; from which the Contraction of the Muscle proceeds. When the Contraction is finish'd, the pure Spirits, which remain, recede, for the most part, into the *tendinous * Fibres*, the other Particles remaining among the *Flesh*; the Blood supplying the Expence of these, and the Nerves of the others. And as to the Instinct or Disposition to the Ordination of Motion, our Author thinks that to be produced by *other Spirits*, sent out from the Brain to the Muscles when Motion is requir'd, which, by their various Impulse, ordain those Spirits, already placed in the Tendons, to different Motions either of Expansion or Recess. This is the Scheme for the Performance of Muscular Motion, according to this learned Author; the chief Part of which is the Agency of Fermentation; wherein there appears but very little Difference between this and the Doctrine laid down by Dr. *Croune*. There seems however some Impropriety

* Here are clearly Dr. *Croune's* three Kinds of Spirits; those in the *Muscular Flesh*, those of the *Tendons*, and those sent from the *Brain*; besides the Fermentation they produce by meeting in the *Bellies of the Muscles*.

priety in imagining, that, when fleshy Fibres are fill'd or puffed up by Fermentation, they should be subject to any kind of Corrugation; it being rather an Effect of Emptiness than Repletion in flexible Bodies.

XXII.

This Hypothesis, however ingenious in the Whole, does not seem very satisfactory; and indeed it was soon taken into Consideration by the famous Dr. *Mayow*, who rejects both this and the Opinion of *Steno*; urging many Arguments against the Sufficiency of either in accounting for *muscular Motion*. Let us see how far he excelled them on the same Subject himself.

XXIII.

This ingenious Author * takes notice of *Mayow*. two Sorts of Fibres; *viz. muscular Fibres*, and membranous *Fibrillæ*: The former he describes with *Steno*; being satisfy'd with what that Author has said about them, as to their Structure:—The latter, being the *Fibrillæ*, he says, are a wonderful Series of Fibres parallel to each other, and which intersect the fleshy Fibres in a transverse Direction, yet somewhat obliquely. This Observation he has made upon boil'd muscular Flesh; and says also, That altho' it had been the Opinion before him, that Contraction was performed by the muscular or fleshy Fibres, yet it is his Opinion, that the *Fibrillæ* are principally con-

* *Tractatus quinque Medico-Physici. Vide De Motu Musculari, cap. 2, 3, 4, &c.*

concerned in that Action : For that, in order to a due Contraction, if it was made in the fleshy Fibres, they must of Necessity be much more shorten'd than the intire Muscle itself; because they are not disposed according to the longitudinal Direction of the Muscle, but are inserted obliquely into the Tendons : And also that the Muscle would swell to an immense Size; which, he says, does not happen upon muscular Motion. Hence he concludes, that the Motion is performed by the membranous *Fibrillæ* ; which, being contracted, draw the fleshy Fibres more closely together, and render the whole Muscle shorter and more hard.

XXIV.

It is remarkable, that in *Plate 3. Fig. 2.* of this Author, the Figure he gives to represent the Situation of the fleshy Fibres, and the *Fibrillæ* that intersect them, is copied by Dr. *Stuart, Plate 2. Fig. 2.* with this Difference, that *Mayow* calls the minute intersecting Fibres, membranous *Fibrillæ* ; whereas the former calls them nervous *white Fibrillæ* * ; and, in his third Figure, supposes each carnosus red Fibre to have a Chain of *Vesicles*, which represent a String of Beads or *Necklace*. Now, as to these *Vesicles*, our Author seems to have given the Hint to some of those

* These are no more than the Fibres of the Membrane that invests the muscular *Fibres* mention'd by *Steno*, and which really exist ; for, in tearing asunder, with one's Fingers, the Fibres of a boiled Muscle, they are very apparent ; and seem to be what *Bernoulli* imagines to bind his fleshy Fibres at equal Internodes, of which more hereafter, being first thought of by this Author, (*Mayow*) as Agents in muscular Motion.

those that wrote on the same Subject afterwards : For, where he speaks of the Manner of the Circulation of the Blood thro' the Muscle, against some Opinions before him, " that it is extravasated from " the Arteries, and absorbed by the Veins," he says, That the Veins and Arteries meet by the Intermediation of Vesicles, which he calls, *Collectio Vesicularum sanguiferarum* * ; that no such Extravasation can happen, and that this Collection of *Vesicles* constitutes the chief Part of the muscular Flesh ; and also, that their chief Use is, like a Strainer, to separate from the Mass of Blood certain Particles, necessary towards the Contraction of the Muscles. This, if maturely considered, will be found to square pretty much with *Keil* and *Stuart*, as to their *Vesicles* ; as will hereafter appear, when we speak of these several Authors.

XXV.

He also agrees in other respects with some of his Predecessors ; particularly about the Necessity of an Effervescence, being raised in a Muscle, necessary to its Motion ; occasioned by the Admixture of Particles
of

* Which indeed do not exist, but are however first thought of by this learned Author ; who also finds it necessary to bring to his Assistance the *Fermentation* of the most worthy Founder of these Lectures ; yet differs from him as to the Nature of Animal Spirits ; the latter giving them the Name of a *Liquor exquisitely impregnated with a volatile Salt and Oil* ; and the former calling them *nitro-aerial Particles*, which ferment by mixing with the *salino-sulphureous* Particles of the Blood : Both which Terms seem to be the Invention of Dr. *Mayow*.

of different Natures, according to the Power of the Will; which proceed from the Brain and Mass of Blood. Those from the Brain, he says, are *nitro-aerial* Particles, and the true Animal Spirits; and those from the Blood he calls *salmo-sulphureous*; and that the former, being sent by the Nerves, meet the latter in the Muscle wherein they are secreted, as was said before, and make the Emotion and Fermentation, which is the Cause of muscular Motion.

XXVI.

As to his Manner of muscular Motion's being brought about, it seems to be intirely his own Invention: He denies that it can be performed by Inflation, either of the muscular Fibres, or *Fibrille*; the latter of which, according to him, are solid Bodies, and can undergo Contraction no other way than by Twisting or *Contortion*; and that those *nitro-aerial* Particles are very fit to affect the *Fibrille* in that Manner, To prove this, he brings the following Experiment: Let a small String of a musical Instrument be held between the Fingers of each Hand, at a considerable Distance from each other, over a lighted Candle, so as that it may become sufficiently heated without burning: When thoroughly hot, it will be perceived to contract with a considerable Force, by twisting itself; and, moved from the Candle, will be easily distended again, by untwisting. Thus, says he, the *nitro-aerial* Particles issuing from the Candle are the Cause of the Contraction of the String; as they are of the *Fibrille* being writhed and shortened about the muscular Fibres in the Body; which being by that means drawn closer together, the whole Muscle is shorten'd.

This

This Experiment, he says, is the more to be relied on, because, by Microscopical Observation, he pretends to have found these *Fibrillæ* exactly like a fine String of a musical Instrument. From this System he concludes, that, as some Force is necessary to dis-tend the String to its former Dimensions after Con-traction, if no Force is applied, it will always remain contracted; and that therefore, when there happens a *Paralysis* of a Muscle, its Antagonist is convulsed, or is spasmodically affected. Thus much is sufficient to shew what our Author's Notions were concerning this difficult Subject. The next we shall consider is the famous *Borelli*; a Man famous indeed for his Calculations of the Powers of moving Bodies, but much less so for his Account of the Cause and Manner of the Motion of Muscles. The following is the Substance of his Opinion concerning that Particular.

XXVII.

He supposes *, that, within the Mem- *Borelli.*
brane which invests a Muscle, the *Fasci-*
culi of muscular Fibres have a prismatical § Form;
which is sometimes triangular, sometimes square, and
sometimes

* Johan. Alph. Borelli *De Motu Animalium*, Pars prima, c. 2. Prop. 1. & c. 17. Prop. 114, 115, 116. Pars altera, c. 3. Prop. 22, 23, 24, &c.

§ Our Author thought so, because he made his Observations upon a dry'd Ham, which, having its Moisture exhaled, and its Fibres being collected, by the Salt used in preserving it, into Bundles, will flake off in Parcels when boiled. But it must be obvious to every one, that Salting, or Boiling, will alter the Contexture of Fibres so minute; and, consequently, that no great Truth can be drawn from such Observations.

sometimes hexagonal; each of which is composed of many Filaments, or tendinous Fibres, which are parallel to one another in every little Bundle or Fascicule, and adhere together by a tenacious *Gluten*, if they are not continued to the Extremities of Tendons, or Membranes; and sometimes are immediately connected to Bones, or carnous Fibres.

XXVIII.

That besides, these Bundles are every-where invested and bound together by innumerable transverse Fibres *, as it appears in a Muscle boil'd, and immediately dry'd; which nervous Fibres seem to compose certain reticular Membranes, together with the Capillary Vessels § that bring Blood to them, and carry it back again; and that these Fibres are nervous, he conjectures from their being very hard and tough.

XXIX.

His Description of a muscular Fibre, which he has observed after being boil'd, is, that it seems, by the Help of a Microscope, to be a *Cylinder* like the Twig of a Tree, not hollow, as a Reed is, but is observed to be full of a medullary Substance, which ought to be spongy like the Pith of Elder; because every soft Twig, which is fill'd with any adventitious Moisture, grows turgid, and is necessarily porous, since it is
fill'd

* These are no other than the Fibres of the investing Membrane mention'd under *Mayer* in Note (*) *Self*. 24.

§ This reticular Structure is made use of by Dr. *Stuart* in his Explanation of muscular Motion; which see in his Turn.

fill'd with Particles of Water as with Wedges, as it appears in a wet Ropè. He seems to be further confirm'd in this Notion, because he observed, in the Fibres of a Piece of *dry'd Ham*, certain sanguineous Particles, or strait and transverse Filaments, disperfed like *Porphyry* or Marble; which, says he, seems not possible to be so, if the internal Substance of the Fibres were not spungy.

XXX.

From his Notion of this spungy Contexture of the internal Substance of a muscular Fibre, he is led further to *imagine*, that it consists of Pores of a rhomboidal Figure, so as to resemble a Chain of *Rhombus's*, which are capable of Contraction like so many Bows, by the Help of the *moving Faculty*; and that each of these *Machinæ*, or rhomboidal Pores of the fleshy Fibres, are so minute, that their Length does not exceed the twentieth Part of an Inch. Hence his Definition of a Muscle is, that its Texture is like a reticular Bundle, composed of rhomboidal Chains contiguous to each other.

XXXI.

A Nerve, he says, is a Bundle or Capillament formed of a Number of fibrous Threads, connected together by a membranous Binding; and that every Fibre may be hollow like a Blood-vessel, altho', from the Imperfection of our Sight, they may seem solid: Yet, if it be not impossible, that they may be Tubes, he had rather believe them little Tubes fill'd with a moist spungy Substance analogous to green Elder, or the
2
like ;

like; because the nervous Fibres are not only soft, flexible, and moist, but also because they admit humid Nourishment, and a Fluid drops from them; all which Properties require spongy Porosities bedew'd with some Liquor.

XXXII.

As to the Manner and Cause of muscular Motion, he allows, that a spirituous Liquor, which in some Places he calls *Substance*, or *Faculty*, passes by the Nerves from the Brain to the Muscle, and that some Fluid, proper to the Muscle, meeting it, something like a Fermentation or Ebullition is excited, which causes that sudden Swelling of the Muscle. The Examples he gives to render this familiar to the Reader. are the Spirit of Vitriol poured upon Oil of Tartar, or any acid Spirits mix'd with fix'd Salts. Hence we see from whom this Author has been furnish'd with his Opinions concerning muscular Motion; *viz.* the *Rhombus's* from *Steno*, with a very little Difference; and the Fermentation from the memorable *Croune*; notwithstanding his being frequently quoted as the real Inventer of these Notions.

XXXIII.

Bernouilli. The celebrated *Bernouilli* *, altho' confessedly a Follower of *Borelli* on this Subject, as it appears in his own Words, where he says, " In which (Account of muscular Motion) I shall tread in the Track of the incomparable *Joh. Al-*
" *phonjus*

“ *phonsus Borelli*, embracing his Hypothesis:” Yet blames him for imagining that the *Machinule* in the muscular Fibres were of a rhomboidal Figure; and will appear to have made no other Alteration, even in what he finds Fault with in that Author’s *Rhombus’s*, than to cut off their lateral Angles; and by that means reduce them to elliptical Forms bound together, at their Extremities, very regularly by transverse Fibres.

XXXIV.

The Account he gives of the Structure of muscular Fibres is the same with that of the foregoing Author, so needs not be repeated here: We shall therefore only give the Reader an Abstract of some Sections of his *Dissertation on muscular Motion*, which regard the Part of this Subject we are at present concerned in.

XXXV.

In his second Section, after he has mention’d *Borelli’s* Inspection of a boil’d Muscle, he says, The Fibres of the little Bundles are collected together by transverse Fibres, which are parallel to each other, and form with the former a reticular Texture; to which he ascribes no other Use, than to confine the moving Fibres, lest, in performing their proper Action, they should be forced too far asunder * from their
natural

* We must here assent to *Bernoulli*, that the membranous Fibres, which not only inclose every Muscle, but proceed to invest every Fibre,

natural Situation. Thus every moving cylindrical Fibre, by the Help of this Ligature, is divided into equal *Internodes*, forming *Vesicles*, which are flaccid when the Muscle is inactive, but when acting, are distended, acquiring an oval Figure like the *Rings* of a *Chain*, which, says our Author, *Borelli* falsely calls *Rhomboidal Machinulae*: And also adds, that these transverse Ligatures are loose enough to admit a free Communication for the moving Matter to fill the *Vesicles*.

XXXVI.

His third Section is chiefly taken up in the Confutation of *Steno's* Opinion, That muscular Motion is perform'd without the Access of any thing whatsoever, but by the sole Disposition in the Fibres to change their Figure, from an oblique-angular Parallelogram into a more strait one; and opposes to it that common physical Axiom, "*Omne quod movetur, movetur ab alio.*" And, in the fourth Section, assents to *Dr. Croupe*, with *Mayow*, *Willis*, and *Borelli*, that Motion must be caused by a *Fermentation* raised in the Muscle; believing also with the latter, that the Nerves are a *Congeries* of Tubes fill'd with a spongy Substance, which are always full of a very spirituous Juice, supplied by the Brain, of such a Nature, as
(when

TO THE

Fibre, serve only to keep them in their natural Situation; but that they divide them thus into Bladders at equal Internodes, we must deny; inasmuch as we have been so happy as to separate distinct muscular Fibres, and demonstrate them before the learned *Royal Society*, and to many Anatomists since these Lectures were read. For their proper Description, see *Lect. 2, Sect. 3, 4, &c. TAB. I. Fig. 1. &c.*

(when mix'd with the Blood) to be capable of raising a Fermentation : This is the Animal Spirit. So that, when the Soul performs an Act of Volition, there must of Necessity happen a certain local Agitation of animal Spirits in the Brain, from the great Union between the Soul and Body ; whereby the Beginning of some Nerve receives an Impulse, which is continued to the Juices thro' its whole Length : And so, from an Irritation at the Beginning of the Nerve, the last Drop of the nervous Juice is, by a gentle Vibration, thrown out at the other Extremity, and also from the little Mouths of every other Nerve dispersed through the whole Muscle in the same manner, according to the Power of the Will. And as a Sponge fill'd with Liquor suffers not a Drop to fall out, so, says our Author, altho' those little Mouths of the Nerves (*in the Muscle*) are always open ; yet the Drops of the nervous Juice never fall from them, without an actual Impulse or Concussion ; because the spongy Substance of the Nerves serves them instead of Valves*.

XXXVII.

When therefore, says he in his *fifth* Section, by the Command of the Will, or from Nature's Custom (in involuntary Motion), innumerable Drops are thrown out together from the Orifices of the Nerves, thro' the intire Bulk of the Muscle, which is always thoroughly moistened with Blood ; then these spiritu-
ous

* This is also taken from *Borelli*, where he speaks of a Nerve.

ous Particles or Drops, by striking their sharp *Spicule* into the more fine Particles of the Blood, break them, and give the confined condensed Air Room to expand itself, and cause the subsequent Ebullition and Inflation of the Muscle.

XXXVIII.

But, because an Objection might lie against this System, as, How it comes to pass, that the Muscle should so suddenly grow slender, and reassume its former State; since, according to our Author's Doctrine of Fermentation, it should seem, that, after the first Ebullition, the Muscle ought to remain constantly swell'd; he proceeds to remove that Obstacle in his *sixth* Section, by having recourse to the following Hypothesis: Let us suppose, says he, besides the thick Air we breathe, another more subtile Air; which, however elastic, can by no means be perceived, as being capable of penetrating freely all the Pores of the Body. He thinks this Supposition not at all absurd, as believing that there is other Matter of different Degrees of Subtility, between this grosser Air of the *Atmosphere* and the *Materia subtilis*, lest there should be a *Chasim* in any Part of Nature; and thinks it must be elastic, from the same Cause that that of the Atmosphere is; to wit, from the continual Motion of the *ethereal Matter*, which always endeavours to drive the less agitated and gross Particles from each other, and thereby obtain for itself a free Passage.

XXXIX.

XXVIX.

This being pre-supposed, says he, the Particles of the spirituous Juice of the Nerves are so very subtile, delicate, and tender, that their *Sinacula*, which at the most gentle Touch are blunted, are only capable of opening the most minute Pores of the Particles of the Blood, from which immediately that more subtile elastic *Aura*, that was condensed before, rushes forth, and expands itself, thereby swelling the Whole at once; but, because of the exceeding Minuteness of its Particles, it freely breaks out thro' the open Pores of the Muscle, and flies off into the ambient Air. Hence the Muscle must of Necessity grow lax in a Moment after its Intumescence; unless new Drops of the nervous Juice continually fall into it, creating a Succession of Fermentations, and so keep it in a constant State of Inflation*.

XL.

This is the Sum of the Opinion of that great Genius the famous *Bernouilli*, touching the Manner in which muscular Motion is performed: The next is that of one as eminent, the learned *Keill*, but whose Sentiments of the Matter agree well with those before him. He defines a Muscle to be “ a Bundle of
“ parallel Plates of fleshy Fibres, which are com-
“ posed of other smaller Fibres, and each smaller
“ Fibre

* We shall find the following Author says no more than *Bernouilli*, explaining it only a different Way.

“ Fibre to be a String of Bladders or Vesicles, into
 “ which, he supposes, the Nerves, Veins, and Arte-
 “ ries to *open*.

Keill. “ That the Contraction, or Swelling of
 “ the Muscles, is performed by the Blood
 “ and Animal Spirits distending these Vesicles ;”
 but endeavours to prove, by many ingenious Argu-
 ments, that neither the Spirits alone, nor the Quan-
 tity of both together, distend the Vesicles ; but that
 both mixing and rarefying * together, cause them to
 swell : For, “ That the Globules of Blood continu-
 “ nually circulating through these Vesicles of the
 “ Fibres, which are, probably, capable of containing
 “ only one Globule at a time, in which Globule (he
 “ supposes a Globule of Air) meet with the Animal
 “ Spirits which drop from the Nerves : That the
 “ Spirits surrounding the Globule of Blood must at-
 “ tract the Particles of it, of which they are com-
 “ posed, more strongly than the others of the Glo-
 “ bule of Blood ; and, consequently, their *Nisus*
 “ to one another ceasing, the condens’d Globule of
 “ Air will expand itself with a very considerable
 “ Force ; whereby each Vesicle of the Fibre will be
 “ distended,

* What the foregoing Authors call Fermentation, Effervescence, &c. this Author calls Rarefaction. *Bernoulli* endeavours to explain it by the *Spiculae* of the nervous Juice striking against the finer Particles of the Blood ; and this Author, by the Attraction and *Nisus* between the Animal Spirits and the Drop of Blood when they meet in the Vesicle : So that, in the Whole, they may be consider’d to speak the same thing (and even not to differ much from their Predecessors) ; for it is no great Matter whether the Globule of Air is freed from its Confinement, by *Spiculae* opening the Pores of the Blood, or by the Attraction of Particles to each other.

“ distended, and, consequently the Fibre shorten’d,
 “ or the whole Muscle will be contracted.

“ But, when the Particles of the Globule of Blood
 “ are mixed with the nervous Fluid, they will both
 “ together inclose the Globule of Air again, and com-
 “ press it into as small a Space as it was in before :
 “ And thus the Contraction of a Muscle must imme-
 “ diately cease, unless fresh Blood and Spirits, suc-
 “ ceeding one another, continue the Swelling of the
 “ Vesicles *.”

XLI.

This System, however ingenious, as it admits of so many bare Suppositions, upon which these Authors found their Arguments, it will be difficult to think it the true Explanation of muscular Motion : But admitting every Supposition to be true, yet the Time that this kind of *Nisus*, *Attraction*, *Rarefaction*, *Fermentation*, &c. must necessarily take up in the Performance, can no way be accountable for the quick Motions performed by the Muscles of the
 Organs

* The only Difference that seems to be between this and the foregoing Author is, that the *Aura*, after being let loose, and swelling the Muscle, flies off into the open Air, according to *Bernouilli*, and the Muscle ceases to swell : Whereas our present Author says, his Globule of Air is again condensed into as small a Space as before, and the Swelling and Contraction of the Muscle ceases : However, their Conclusion is the same for the Continuance of the Motion or Swelling ; this Author requiring fresh Blood and Spirits succeeding one another ; and the foregoing Author the same, in these Words :—*Musculus iterum detumescat, nisi jugiter novæ instillantur Guttulæ Succi nervosi, quæ novam & novam pariendo Ebullitionem, Musculum in continua Inflatione conservent.*

Organs of Speech, the Twinkling of Eye-lids, or any others that are done as quick as thought, no more than it can be reconciled to the Nature of involuntary Motion, in those Muscles that are not subject to the Impulse of the Will.

XLII.

His Explanation of Rarefaction, admitting we expected no Use of it in muscular Motion, seems to be very particular too; for we can scarce have any other Cause in View for Rarefaction or Condensation, than *Heat* or *Cold*; which are sufficient of themselves to produce those Effects in the most *homogeneous* Fluid, without the Admixture of any other. Whether Rarefaction can be effected by any other Cause in the Body, will be hard to determine, if we do not suppose those Spirits to be an acrimonious Fluid, capable of causing a Commotion with the Globules of Blood in the Vesicles, which would amount to a *Fermentation*; but our Author says all is done without Fermentation, by this methodical *Mixtus*, and Attraction of the Particles of the Spirits to each other.

XLIII.

Quincy. I must here insert another very remarkable Notion propagated by *Quincy*, from a Hint of *Bellini*, as it appears in his Explanation of the eighth Aphorism of *Sanctorius's* second Section, where he owns his being led by * *Bellini*, in his
Notions

Notions of the Structure and Power of a distractile Fibre. He supposes the Parts of a distractile Fibre to be made up of certain *Machinulae*, like *Syringes* and their *Embolus's*, and their Motion to be analogous to that of the Instrument mention'd. " The Observations and Experiments (says he) which have been made of late, but more particularly by Mr. *Boyle*, about the Spring of the Air, have explained to us the Contrivances and Properties of a *Syringe*; and the Reasons upon which that Phænomenon of the Difficulty of drawing back the *Embolus*, when the Pipe is stopped, depends; and the Necessity of any Liquor's following it, wherein the Pipe is immersed: The Reason of which being well considered, it will be found, that all which is necessary for this Contrivance is, that the *Embolus* be so exactly adapted to the inner Surface of the Barrel, as to prevent any Air passing between them when it is drawn up; and that it matters not what Figure the Barrel is of, so that the *Embolus* is well fitted to it. It easily therefore might be contrived to make a Case of Syringes, wherein every Barrel may also serve as an *Embolus* to its Exterior, which immediately includes it. And, with this View, it is not at all difficult to imagine a continued Series of Particles so put together, that the inner may be moved and drawn upon one another, without suffering the Air immediately to enter into the Interstices made by their Distraction: Whereupon, as soon as that Force which drew them is removed, they will, for the very same Reason as the *Embolus* of a *Syringe*, rush up again into their former Contacts." This is his Notion of the Arrange-

ment of the Particles composing the main Substance of an animal Fibre, and of its Construction.

Now it is no difficult Matter to overturn this System, since there are two principal Objections to be made to it, which prevent the Trouble of any further Consideration. The first is, That this absolutely is not the Structure of a muscular Fibre, as we shall make it appear in the following Lecture: And, secondly, There is a great Absurdity in the Comparison he makes between the Action of a *Syringe* and its *Embolus*, and that of a *flexile* fibrous Thread: For, as it is necessary, in the Action of that Instrument, that its Sides should be inflexible, hard, and able to bear the excessive Force of Suction in the Retraction of the *Embolus*, and the Force of the Air in its Repulsion, with how little Propriety can this minute supple Fibre of a Muscle be said to be capable of a like Action, or such a Wedge-like Construction as this Author thinks applicable to it.

When, in mechanical Reasoning, we find it necessary to illustrate an Opinion by a Comparison, there ought to be a true Similarity of Structure in the Objects whose Actions are to be compared; otherwise it would be altogether as prudent to liken a human Blood vessel to a Blunderbuss.

XLIV.

Monro. The ingenious Professor * *Monro*, of *Edinburgh*, supposes the nervous Fluid to be
 “ an extreme fluid saponaceous Water, flowing in a
 “ con-

* *Anatomy of human Bones and Nerves*, p. 29 of the latter.

“ constant, equal, *slow Stream*, from the *Encephalon* and *Medulla spinalis*, in each of the proper
 “ nervous Fibres, &c. That it is fit for nourishing
 “ and restoring the Particles that are constantly carried
 “ away from the Solids by the *Vis Vitæ*, Circulation
 “ of the Liquors, and necessary Actions of Life; and
 “ that the constant Flow of the Liquor of the Nerves
 “ into the Cavities of the muscular *Fibrille* occasions
 “ the natural Contraction of Muscles, by the constant
 “ *Nisus* it makes to increase the transverse Diameter,
 “ and to shorten the longitudinal Diameter of each
 “ Fibre; and that it is only to allow the Mind a Power
 “ of pouring a greater Quantity of this Fluid, with
 “ greater Velocity, into what muscular Fibres it
 “ pleases, to account for the voluntary strong Action
 “ of Muscles.”

XLV.

To this Opinion we will here subjoin *Robinson*,
 that of Dr. *Bryan Robinson* *; who, after
 taking much Pains to prove, that a Hair or Fibre de-
 creases in Breadth as it increases in Length, and *vice*
versa, concludes that, “ if a vibrating Motion be
 “ raised in the *Æther* contained in the Fibres of
 “ Muscles, the Diameters of all the Fibres will be
 “ increased, and their Lengths lessen’d by that Mo-
 “ tion: All therefore that is necessary to give Ani-
 “ mals a Power of moving their own Bodies, is to
 “ give them a Power of raising a vibrating Motion
 “ in the *Æther* contained within the Fibres of the
 “ Muscles.

* Dissertation on the *Æther* of Sir *Isaac Newton*.

“ Muscles. And this Power they have by the Me-
 “ diation of the Nerves, which are solid uniform
 “ Threads, arising from that Part of the Brain to
 “ which the Soul is present, and terminating in
 “ the Muscles: For, a vibrating Motion, rais’d, by
 “ the Power of the Will, in the *Aether* contain’d
 “ in that End of a Nerve which terminates in the
 “ *Sensorium*, or Place in the Brain to which the Soul
 “ is present, will, in an Instant, be propagated to
 “ the Muscles supply’d by that Nerve, and raise a like
 “ vibrating Motion in all its Fibres, from the very
 “ great Communication there is between the Nerves
 “ and the fleshy Fibres of the Muscles.”

XLVI.

At to what Professor *Monro* says of the compo-
 nent Parts of the nervous Fluid, it would be fit for
 the Purposes he assigns, by a good Analogy drawn
 from Plants, *viz.* to nourish and supply wasted Par-
 ticles, as well as to propagate muscular Motion; pro-
 vided it was of a Consistence subtile enough to pass
 thro’ those extremely minute Tubes, and that the
 Mass of Blood was not the only *Pabulum* design’d
 by the Creator for affording Nutrition. There is
 great Need, in whatever Fluid the Nerves contain,
 of a Capacity of acting with the greatest Celerity
 imaginable; and one would think a Mixture of a
 saline and oleaginous Matter not the most fit for such
 active Performances as the Will sometimes deter-
 mines, and some of which are as swift as mere Ex-
 plosions. However this be, our ingenious Author

has not so much made it his Business to account for the Manner in which this Fluid causes the muscular Fibres to increase and swell, in order to the Contraction of the Muscle, as to give the true Description and Situation of the nervous System; than whom none has done it better.

XLVII.

And as to the Opinion of Dr. *Robinson* concerning the *Æther*, it can hardly give Satisfaction: For supposing this *Æther* to be the Cause of muscular Motion, there must be a *Modus Actionis* to produce the necessary Effect; for it is not enough to say a vibrating Motion causes the Muscle to contract, without making some Attempt to shew in what Manner it does so. We can understand as much, by saying at once, The animal Spirits cause the Muscle to move; which was said many Years ago.

But, from a very natural and obvious Argument, one may venture to doubt, whether the *Æther* can have any Share in muscular Motion; for there can be no Motion, whatever, mechanically performed in One Body, without first receiving a certain Propulsion from something else; and the Body making that Propulsion ought to be endow'd with Qualities necessary for such a Resistance, proportioned to the Body acted upon: Now, if the *Æther* (as Philosophers have defin'd it) be a *subtile Matter* pervading all substantial Bodies, of whatsoever Solidity, without Lett or Hindrance, it cannot be said to cause
muscular

muscular * Motion, since it can meet nothing to oppose its Passage; and, consequently, can make no Resistance.

XLVIII.

* Whatever Necessity there is for the Existence of the *Aether* in the Oeconomy of the World, we can hardly suppose it either the *Cause* or *Instrument* of muscular Motion; for the *Soul*, or *Mind*, seems to be the very Cause of voluntary Motion: And as to its being instrumental, it will be extremely difficult to reconcile the Qualities of the *Aether* to those of the Parts we shall, in our next Lecture, endeavour to prove are the Instruments of that Motion.

The excessive Distance between the Subtlety of this *Element*, and the Grossness of the *nervous* and *muscular System*, upon which they are, by this Author, supposed to act, will admit of no Proportion: Wherefore, to suppose the Nerves can be impressed by the *Aether*, we must pre-suppose Millions of Gradations of grosser Particles from the extreme Fineness and Subtlety of this, down to the visible Grossness and Solidity of the Organs to be moved, in order, as it were, to hand down the Impulse from the extreme subtle to those extreme gross Particles: All which raises in me so complex an Idea of that Impulse, that I cannot find it compatible with the great Quickness of both the Resolution and Impulse we daily see in the Performance of animal Motions.

There is another Argument, which seems very powerful against the *Aether's* being instrumental in muscular Motion, drawn from some Considerations on the Fire produced in the *electrical Experiments*, now verif'd by Mr. *Watson*, a worthy Member of the *Royal Society*, provided this *electrical Fire* be analogous to the *Aether*; which is, That it is certain, this Fire pervades animal or other Bodies, from my own Experience; as I was one of several Persons thro' whom it passed, without having any sensible Effect on me, in its Passage to the Point where it was collected into a Body: And also, because silver *Laminae* were moved by the excited Tube, even thro' the Sides of a strong Flint-glass Vessel well stopp'd.

It seems, indeed, a very great Care in the All-wise Author of Nature, that this *electrical Fire* should not find Resistance in animal Bodies; because, we see, when all that passed from the excited Tube, through several Persons, was collected in one Column at the End of a Gentleman's Finger, it fired rectify'd Spirits of Wine, and Oil of Orange-Peels; and, consequently, might produce as direful Effects as the Fire of *Lightning*, when collected and excited to violent Motion, and is resisted,

XLVIII.

The last Opinion I shall trouble you with *Stuart*. is that of my Predecessor in these Lectures, the late learned Dr. *Stuart*; who (in his Explanation of the Experiment upon the Frog, to which I refer the Reader) concludes, “ That voluntary muscular Motion is begun by the Impulse of the Mind or Will on the animal Spirits, thro’ the Nerves into the Muscles.”

He supposes (with *Keill*) the Structure of a muscular Fibre to be vesicular, with a reticular *Plexus* of Blood-vessels investing each Vesicle: His particular Definition of it is this: “ It is a nervous Fibre, produced from its Entrance into the Muscle, along, or in the Axis of each carnosus Fibre, in the Form of a Chain of distensible Vesicles, whose Sides are cover’d with a Net-work of elastic longitudinal and transverse Blood-vessels, &c.” Here he makes a Difference between the nervous Fibre in the Form of a Chain of Vesicles, and the carnosus Fibre, along whose Axis it is produced; whereas Dr. *Keill* says, Each Fibre is a String of Vesicles. Our Author endeavours to explain his System by these little longitudinal and transverse Blood vessels on the Surface of each of these Vesicles, which he calls the reticular *Plexus*. I must confess I do not well comprehend his Meaning; however, the Sum of his Account is as follows:

“ In the utmost State of Extension (of a Muscle),
 “ the longitudinal capillary Blood-vessels on the Surface of each Vesicle in the Fibres must be extended,
 “ and therefore their transverse Diameters must be
 “ lessened;

“ lessened; that is, these Vessels thereby become
 “ straiter, and the Circulation in them therefore
 “ more difficult; and in this State also the transverse
 “ Blood-vessels of each Vesicle will be forced into
 “ serpentine Flexures, which must render the Passage
 “ of the Blood thro’ them still more difficult.

“ If the Mind impels but a little more of the
 “ nervous Fluid than usual thro’ the slender Tubes of
 “ the Nerves into these extended Vesicles, they will
 “ be uniformly dilated.

“ By this Distension of the Vesicles, their *Axes*
 “ being shorten’d, and their Diameters lengthened,
 “ the longitudinal capillary Vessels in their Surface
 “ must be shorten’d, and thereby their Diameters en-
 “ larged, and the serpentine Flexures of the trans-
 “ verse Vessels will be extended; which, in both Kinds,
 “ will lessen the Resistance they gave to the *Transit*
 “ of the Blood; which, by the *Diastole* and *Systole*
 “ of the Arteries, is continually urged on to its Pass-
 “ age thro’ them; and, being thus facilitated, every
 “ Globule of Blood in its Progress, by endeavouring
 “ to fly off by the *Tangents* of these Vessels and Ve-
 “ sicles, tends to expand them more, and thereby
 “ opens the Way for the further and easier Influx of
 “ the nervous Fluid, to which the Blood vessels con-
 “ tribute, as so many elastic Levers acted upon by
 “ the Blood in its Progress. Thus, by the Assistance
 “ of these three Powers, the nervous Fluid, Blood,
 “ and Blood-vessels, the Progress from Extension to
 “ *Diastole* of the Vesicles is made, by which the
 “ Muscle becomes tumid and enlarg’d in Bulk, &c.

“ But, if the Mind desists sending this Recruit, or
 “ suspends it, then these circular arched elastic Ves-
 “ sels,

“ fcls, now turgid with elastic Blood, whose *Area's*
 “ have been thus forcibly enlarg'd, endeavour to con-
 “ tract themselves every Way towards the Centres of
 “ their *Area's*, which are the Centres of the Vesi-
 “ cles; and the Mind giving no Resistance, this *Nisus*
 “ takes Place, to the complete Contraction of each
 “ Fibre; by which means the Limb affix'd is brought
 “ into complete Flexion, or Extension, according as
 “ this or the other Antagonist has been acted upon.

“ In this State the whole Muscle becomes shorter
 “ and less in all its Dimensions; harder and paler by
 “ Expulsion of a great Part of its Fluids thro' the
 “ Veins towards the Heart, and thro' the Extremi-
 “ ties of the Nerves into the Tendon and *Peri-*
 “ *osteum*.”

Here he supposes the Spirits to fly off to the Ten-
 dons and *Periosteum*.

XLIX.

None of all these Authors have consider'd the State
 of a Muscle when at Rest; which has contributed not
 a little to assist my Explanation of muscular Motion.
 Our Author has nothing in his View, but the bare
 Progress of the Motion from the utmost Extension to
 the utmost Contraction of a Muscle; and, in the Ex-
 planation, the whole Progress seems intricate and te-
 dious. Again he supposes these longitudinal and trans-
 verse Blood-vessels to be on the Surface of the Vesi-
 cles, which do not appear by any Assistance of the
 Microscope we can apply.

L.

He begins this Explanation with the utmost Extension of a Muscle; and endeavours to prove, that, in its Progress to Contraction, the whole Muscle must grow larger in Bulk every Way, by shortening the *Axes*, and lengthening the Diameters of the Vesicles. This he calls their *Diaſtole*, which happens from the Mind's impelling more than ordinary of the nervous Fluid; but says, That, when the Will ceases that Recruit, the complete Contraction follows by the *Nisus* of the circular Blood vessels towards the Centre of their Vesicles.

LI.

Now one would be apt to conclude, from this Way of Reasoning, that the Vesicles ought to lengthen again by the Restriction of the circular Blood vessels; for, as their Diameters were lengthened by the Contraction of the longitudinal ones, so, by the Contraction of their transverse or circular fibres, the Length of the Vesicle ought to be increas'd again; and, consequently, the Muscle ought to return to its State of Extension.

LII.

Here is one short Particular more, that seems unintelligible, with which we shall close this Introduction: He applies his Experiment on the Frog, to prove, that the complete Contraction of the Muscle is excited from the *Impulse* of the animal Spirits into it; whereas here he says, " If the Mind *ceases* " her *Impulse*, the *Nisus* of the circular Fibres of
2
" the

“ the Vesicles takes Place, to the complete **Contra-**
 “ ction of the Muscle.” I submit the Consideration
 of these different obscure Accounts to Understand-
 ings more penetrating than mine; and shall take an
 Opportunity of humbly offering, before this learned
Society, in another Reading, my own Attempt to-
 wards an Explanation of muscular Motion, pursuant
 to their Command laid on me for that Purpose.

The End of the First Lecture.

LECTURE II.

Containing the Author's Scheme of MUSCULAR MOTION.

S E C T. I.

Read in February,
1743-4.

IN my first Lecture, which is an Introduction to this, I enumerated the chief of the Opinions that have been exhibited concerning muscular Motion, with some short Remarks, which I hope will prove sufficient to shew they could not give the necessary Satisfaction: And indeed I am far from imagining, that I have brought the Subject to a *Ne plus*, or that what I shall advance will prove unexceptionable to every one; therefore if I have been so fortunate as to have hit off but one Step, by which any new Light may arise in this obscure Subject, it is all I can expect: In the Prosecution of which I shall avoid all Suppositions, as much as possible, that cannot be drawn from something experimental; and shall proceed in the most clear Manner I am capable of, in order to entertain you.

II.

There is not the least Motion performed in any Part of an animal Body, which does not depend on a muscular Structure for its Progress, whether in the Fluids or Solids, voluntary or involuntary; and therefore whatsoever Explanation of muscular Motion is not reconcileable to, and accountable for, every Motion performed in any Part, cannot be the true Explanation.

III.

Of a muscular Fibre.

The most minute muscular Fibre *, that I was able to separate, seems to be tubular, but unequal; that is, having some Parts of it more protuberant than others, so as to resemble as many Sailors *Hammocks* one after another, and much in the same Proportion in general: We shall call these Bellies or Hammocks, *Cells*, for the better Explanation of the Subject. Now tho' it is a received Opinion, that Fibres are divisible *in infinitum*, that is, that each Fibre is composed of others, and those again of others, and so on; yet this *muscular Fibre*, as it is a *Tube*,
and

* A muscular Fibre of a middle Size is about equal to the Hair of a Child's Head newly born, at the strictures or smallest Parts; the *Cells* being thicker according to the Proportion at TAB. I. Fig. 1, 2, 3, &c. Yet some are larger, and some smaller; the *Cells* however are not to be discerned with a Glass of a less magnifying Power than the Fifth of a double reflecting Microscope.

and serves to perform an Office peculiar to itself, it cannot be divided, without destroying its necessary Form; as a Trumpet, or any other hollow Vessel, cannot be divided or cut into other Trumpets, &c. (let its Size be what it will) and is therefore, properly, an *ultimate Tube* or *Trumpet*, as this tubular Fibre is an *ultimate muscular Fibre*; and this is the Case with the *nervous Tubuli*. Indeed the *Parietes* of every *muscular* and *nervous* Fibre may be composed of Fibres divisible, for what I know, *in infinitum*.

IV.

These Cells are not regularly alike, some appearing three times longer than others, nor at the same Distances from each other; nor do the Fibres themselves seem all of a Size, (*Fig. I. 1, 2, 3, &c.*) and consequently each Fibre contains some more, some less of these Cells.

V.

The Cells communicate with each other by a Passage through the whole Length of the muscular Fibre, till both Extremities terminate in a Tendon or otherwise; which appear'd upon many Experiments both while the Muscle was moist, and after drying a little, having separated them in both Cases; but after being boiled or roasted, every Fibre, I examined, seemed to have lost its Cells, and become uniform; whence one would be apt to think the Cells had burst by the Rarefaction of the *inflating Matter* * they contain'd,
and

* Sect. X. a little further.

and their Sides become uniform with the Parts that were, before, more slender.

VI.

And as these Cells communicate one with another, it may be reasonable to suppose, that there is no Necessity for any more than one nervous Tube to each muscular Fibre; so that the Number of nervous Fibres in that Bundle that goes to a Muscle need only to be equal to the Number of muscular Fibres that compose it. From this Structure it is easy to conceive, how the muscular *Cells* are capable of being shorten'd, by their being inflated * and increased in Diameter: And it is to be further observed, that the Bellies or *Cells* of neighbouring Fibres do not lie regularly by the Sides of each other, but promiscuously; that is, the *Cells* of some lie close to the slender Parts of others, and sometimes two slender Parts lie together, and sometimes two *Cells*.

VII.

Of a nervous Fibre.

The smallest nervous Fibre I was able to separate seems to be a Tube; therefore a Nerve may be defin'd a Bundle of uniform Tubes, whose Sides are parallel

* The Authors, mention'd in my first Lecture, make use of the Word *Inflatio*, and its Verb, to signify a Repletion of the Fibres, or their supposed *Bladders*, *Rhombus's*, &c. with Blood and Spirits, fermented together. On the contrary, I apply it to signify a Blowing up of the muscular *Cells* with an elastic *Aura* only, denying that any Blood or Spirits can get into them. See Sect. X. and XVIII. following.

parallel to, and in Contact with each other, beginning in the *Encephalon* and Spinal Marrow, and terminating in Muscles, Membranes, *Cutis*, &c. for the Propagation of Motion and Sensation *.

VIII.

But because they have not been thought tubular by some ingenious Men, it will be necessary to give some Reasons for our thinking them so. If they are not Tubes, but solid Strings, there is no Way of accounting for the Beginning of muscular Motion, but by their Vibration: Now nothing can be said to vibrate that is not elastic, and first in a State of Tension; but, from known Experiments, there is no Elasticity in the Nerves; nor can any Anatomist say he ever found a Nerve in a State of Tension.

IX.

The GREAT CREATOR seems to have wisely avoided any Tension in a Nerve, for several probable

* Altho' I think it not the Business of this Lecture to touch upon Sensation, yet I am induced in this Place to make the following Reflection: If the *Soul* dwells, or exists in every individual solid Part of the Body (which I am inclined to believe, rather than confine her to any one particular Place), sure she may instantly be apprised of every *Contact* producing the different Sensations, that can happen, wheresoever there is a *nervous Expansion*; without our being under a Necessity (by placing her only in the Brain) of bringing to her Assistance an *Undulation* of animal Spirits to and from the Brain; a Vibration of the Nerves; or, with some Authors, contrary Motions of those animal Spirits in the same Tube, serving to Motion and Sensation; all which are productive of many *Abundities*, which we have not Room here to enumerate.

ble Reasons: 1. The Origins of the Nerves in the *Encephalon* and spinal Marrow are a mere Pulp, very soft and tender, before they unite to form the Nerve, and very loose in their Contexture; so that a Vellication in any one, produced by Tension, would pull it out by the Roots, and would disorder the Parts of its Insertion too: 2. Supposing the Places of the Origin and Insertion of a Nerve to be firmly fixed, and in no Danger from Vellication, there could still be no Tension in the Nerve, because no Nerve could be brought into right Lines; the very Structure of the Parts, through which they pass, would hinder it; for the Course of the nervous Trunks of the Limbs, &c. being along the Interstices of Muscles, &c. if a Tension was produced in them, the Sides of the Muscles, by which they run, would be preternaturally pressed, and become so many Angles or Centres of that Vellication, to the great Detriment of their several Functions: 3. If a Vellication was produced in the *intercostal Nerve* or *Par vagum*, which communicate with several of the principal Nerves in the Body, would there not be great Confusion brought about, in all the Parts to which such communicating Nerves lead; and would not their proper Actions be much impeded?

From these and such-like Reasonings, we must conclude the nervous Fibres to be Tubes, capable of transmitting something to the Parts into which they are inserted, from the Brain and spinal Marrow.

X.

These nervous Tubes contain a Fluid *, whose Nature and Property is not so much to nourish as to inflate ;

* This Opinion leads me to the following Queries:

1. Is it not well known, that confined Air is capable, by a proper Impulse, of performing more violent and swift Motions, than any *Succus* or *Liquor* that can be conceiv'd ?

2. Are not animal Spirits a *Quid occultum* ?

3. Is not the Blood the sole Mass from which every Liquor of the Body is secreted ? If so, may we not as rationally suppose, that any exalted or refined Liquors, for whatsoever Purposes design'd, may be sent off through exquisitely minute Canals, to answer their several Ends, immediately from the Mass, as to *imagine* such are only secreted in the Brain ? For, by the Assistance of the solar Microscope, such exquisitely minute Canals, as could not be discerned even by the double Microscope in a full View of the Circulation in the Mesentery of a Frog, appear'd to be fill'd with a clear Liquor passing rapidly from the Vessels wherein I saw the Blood circulate, and which seem'd to me some Hundreds of times smaller than those Vessels from which they sprung.

4. Is not the Blood full of Air, and is it not daily supply'd by Inspiration in the Lungs ? If so, why may we not as well suppose, that the Blood carries to the Brain, and there deposits, a sufficient Quantity of an *Aura* to supply the Nerves and muscular Cells, in order to their Motion, as we shall explain it by-and-by, as that animal Spirits are secreted there ? any Liquor, as we have said before, being less likely to serve to swift Motions than Air.

Now till the Negative to these Queries can be proved, I cannot help thinking, that nothing but this *inflating Aura* can enter into the Cavities of the muscular Cells ; and consequently must consider the Brain as destined only for treasuring up this *inflating Aura*, and the *inert* Fibres as Pipes to convey it to the muscular Fibres, into whose Cells they alone can open ; that no Nutrition can proceed any other Way than immediately from the Blood-vessels to the Parts that require it ; and that therefore it may, with some Probability, be concluded, that the *Encephalon* and *Spinal Marrow*, *Nerves*, and *muscular Fibres*, are a Set of mechanical Organs, exempt from any other Office, than to cause Motion and Sensation by *Inflation* only, so that every Muscle that moves may justly be said to be blown up

inflate; and consequently whenever the Nerves are impress'd by the Soul, there is an immediate *Inflation* of all the Cells, in the Fibres of that Muscle which is to perform a voluntary Motion.

XI.

And these muscular Cells, together with the nervous Tube that opens into each muscular Fibre, are constantly full of this *inflating Matter* to a certain Degree; that is, fill'd to a *Medium* between their utmost Compression or Emptiness, and their utmost Capacity of Repletion. By this means, the muscular Fibres, when at Rest, are in a *Medium* between Distension and Contraction; but are distensible to near a Third longer, and contractible to near a Third shorter, than when in a State of Rest; the former, by the *Retraction* of Part of the *inflating Matter* back into the Nerves; and the latter, by its *Impulsion* or Inflation into the *Cells* from the Nerves.

XII.

There is no Necessity for imagining, with some Authors, that the Soul makes her Impulse in the Head rather than any other Part, in order to impel from thence the inflating Matter of the Nerves to this or that Muscle: Because, by supposing the Nerves always thus full, the smallest Impulse on the Part of a Nerve leading to this or that Muscle will be sufficient to perform what is necessary; whereas, if we confine the Power of the Will to the Brain, may there not be Danger of Irregularities like Explosions, from thence into the different Divisions of a Nerve, and

to of causing confused Motions in several Parts at a time, like an Epilepsy? Or if an Impulse be made on the Trunk of a Nerve, the Divisions ought a'l to be subject to the Effect of that Impulse. It will therefore be better for our Purpose to think the *Mind* makes her Impulse where she pleases, and chuses that Part of a Nerve only that leads to the particular Muscle to be moved.

XIII.

This seems to be corroborated greatly by the Motions we see in Insects after being cut to Pieces, as Worms, Flies, &c. and in more perfect Animals, as Poultry, Frogs, &c. which move, and shew Signs of Pain, a considerable time after their Heads are cut off; which could not happen, if the Impulse was only made in the Brain. We can carry this Argument yet further, even to human Nature, if what *Diemerbroeck** relates be true: He says he saw a Man executed at *Leyden*, who, after his Head was struck off, rose upon his Feet, and stood for a little Time. And *Dr. Stuart's* Experiment upon the Frog (which is, after the Head is cut off, and the Limbs hanging loose, to compress the spinal Marrow with the End of a Probe made flat, whereby the Limbs are immediately contracted, and with some Violence) shews, that if the Impulse was made in the Brain, the Motion would be confused and general; since all the lower Parts were moved by the
single

* *Anat. corp. hum. lib. viii. cap. i. de nerv.*

single Impulse upon the spinal Marrow of the Frog.

XIV.

Another Reason for supposing the Soul capable of chusing any Part upon which her Impulse may be made, is, that if it was made in the Brain alone, there might be some Impediment or Interruption to her Intention, produced in the *Ganglions* of the Nerves; which, some Authors think, do the Office of so many Brains, but which, more probably, serve as so many *Fulcra* or *Stays* to keep the Nerves firm, and to favour their further Directions to the different Parts of the Body.

XV.

Of Interstitial Air.

The Air-Pump shews us, that, in all flexible Bodies, such as Flesh, small Animals, and the like, there is a sufficient Quantity of Air, lodg'd in the Interstices between the muscular Fibres and the Blood-vessels, &c. to resist the Pressure of the ambient Air; which Pressure being taken off in the Air-Pump, this interstitial Air will expand itself, and swell the Flesh, Animal, &c. to an incredible Size. This may be considered as a general *Equilibrium* kept up between the ambient Air, and that in an animal Body, for the Safety and Preservation of the Animal: And these Air Globules, which we shall throughout this Essay call *interstitial Air*, are constantly in a middle State between their utmost *Contraction* or *Condensation*,

tion, and their utmost elastic *Expansion*, except chang'd into either State by some adventitious Cause.

XVI.

Of the Circulation of the Blood in the minute Vessels.

The Blood appears, by the Microscope, to flow from Arteries into Veins immediately *, Which is easily seen in the Webs of Frogs, Tails of Fishes, *Mytuli*, and the like; and, I doubt not, would appear so in Muscles, if they were thin enough to become transparent for Viewing with Glasses. Therefore the Arteries and Veins may be considered as continued Tubes, terminating in nothing; but as the Arteries arise immediately from the Heart, so they run to the Extremities of the Lungs and Body, ramifying and decreasing in Diameter, till they become invisible to the naked Eye, and gradually become Veins, which unite into Trunks, increasing in Diameter till they arrive at, and open into, the Heart again. So that Nutrition and the Secretions are carried on by minute Twigs, from these continued *capillary Canals* † sent off to the Glands, and to the Parts to be nourished.

XVII.

* Without the Intervention of any Vesicles, such as *Mayer* suggested. See TAB I Fig. 3.
 † Where the Arteries degenerate into Veins.

XVII.

And it further appears, by microscopical Observations, that these capillary Canals run parallel to, and by the Sides of, the muscular Fibres in general, or to Fibres of whatsoever Nature, where these capillary Canals exist, This seems a very wise Contrivance in every Degree; for if their longitudinal Direction was cross the other Fibres, the Circulation could not be so smoothly nor securely carried on; and therefore would be liable to great Impediments, from a transverse Pressure of the muscular Fibres upon them; whereas, in this parallel Direction, they are secured from any Impediment, but what proceeds from the Pressure of the Sides of the muscular Cells upon their Sides, in the Performance of muscular Motion: We are here to take notice, that the muscular Fibres receive Twigs from the Arteries to nourish them, and from the Veins to carry back the *Residuum* into the parallel Canals, as we have hinted before.

XVIII.

Hence we must conclude, that no *Vessel*, of any kind whatsoever, opens into the Cavities of the *Cells* of the muscular Fibres, but Nerves.

XIX.

Of the Equilibration and Motion of Muscles.

From what we have premised, let us endeavour to shew how muscular Motion is performed, and
how

how the several Principles just laid down are applicable to it.

XX.

THE Force of the *inflating Matter* contained in the *Cells* is only equal to the Force of the *interstitial Air*, while the Muscle is at Rest. This may be call'd the peculiar *Equilibration* of a Muscle in itself; but, by the Impulse of the Will on the Nerve, an additional *Inflation* is made to the *Cells*, and then their Force becomes superior to the *interstitial Air* Globules; and again becomes equal when that Impulse ceases, and the Muscle is at Rest. But if, upon the Cessation of that Impulse in one Muscle, there is an Impulse made on the Antagonist at the same time, in order to move it, then the Force of this *interstitial Air* in the ceasing Muscle, exceeds that of the *inflating Matter* in the *Cells*, compressing their Sides, whereby they are lengthened beyond their Tone of Rest; as we shall more fully explain it, when we speak of the *Æquilibration* between antagonistic Muscles.

XXI.

From this *Inflation*, which is perform'd as quick as Thought, the *Cells* instantly increase in Diameter, and grow shorter, compress the venal Canals, and obstruct them. Hence the Blood stops, and the Arteries, by Propulsion from the Heart, increase in
their

their Diameters, while the Veins are squeezed quite empty, and the whole Muscle is shortened, and its Action performed.

XXII.

The Degree of Contraction in a Muscle is always as the Force necessary to perform such or such an Action; and this is determined by the Degree of the *Inflation* of the *Cells*; that is, if the Force required be but inconsiderable, then the Impulse of the Mind will be but inconsiderable, and consequently the *Cells* will have but an inconsiderable *Inflation*; therefore the Pressure of the *Cells* on the minute Veins will be in Proportion: So that of the arterial Blood, tho' it flows always in the same Quantity into the Muscle; yet, because the Pressure of the *Cells* on the Veins is but small, a proportional Part will go on, and return by the Veins; and therefore the Repletion of the Muscle, and its Contraction, can be but inconsiderable: Whereas, when a violent Motion is to be performed, then the Degree of *Inflation* of the *Cells* will be very great, the Blood totally stopped in the Veins, the Arteries increased in Diameter, and the Muscle shortened even to its ultimate State of Contraction, if requisite.

XXIII.

While the Circulation is thus hindered in the acting Muscle, there can be no Disorder occasioned, either in the Oeconomy of the Circulation in other Parts of the Body, nor in the particular Constitution of the Muscle itself. The former Case is certain from our daily Experience of Amputations; wherein,
I tho'

tho' a Limb be cut off, and the Space in it immediately wanting, and although the same Quantity of Blood, supply'd by Digestion every Day, is carried into the Vessels, yet their Distensibility is such, that the only Change they can suffer is further Repletion. And as to the Muscle itself, if, during a violent Action, there should happen any Danger of Injury, it would be perceived immediately by the Mind, and remedied by ceasing her Impulse on the Nerve and Cells, and thereby granting a free Circulation instantly.

XXIV.

As to the *Æquilibration* between two antagonistic Muscles, Dr. *Stuart*, in his Lectures*, thinks, that
 " As each Antagonist has its distinct Nerve or Nerves
 " without Communication, and the antagonist Mus-
 " cles communicate one with another by one com-
 " mon Trunk of an Artery, and one common Trunk
 " of a Vein; they are like two antagonist Scales in
 " *Æquilbria*, over which the Mind has a distinct
 " Power, by distinct Nerves, for determining the
 " animal Spirits, and thereby the Blood, to either
 " Side at Pleasure, without affecting the other: And
 " concludes from thence†, that if what is taken
 " from one be added to the other, the *Momentum*
 " of the Motion will be doubled."

XXV.

* Lect. III. pag. xlii. Sect. 9.

† Sect. 17. pag. xlv.

XXV.

Now there does not seem to be Occasion for taking away any Blood from one Muscle to be determined to the other; nor has the Doctor explain'd how it may be brought about by the Mind; nor, indeed, can I conceive it possible to hinder the Motion of the Blood in the arterial Trunk, leading to one Muscle, while it is sent to the other; except we could suppose the Mind capable of making a Ligature, or other Stricture, on the Ramification leading to it, leaving the other open at the same time. But no Nerve can have such a Power from the Mind to act upon the main Trunk of an Artery; and therefore the *Effect* of the Mind's Impulse upon the Nerve can only be produced, as I have said, on the most minute venal Canals; where they are capable of being press'd by the Bellies or *Cells* of the muscular Fibres that lie by their Sides, and where alone the mechanic Structure of the Parts admits of it: Besides, the arterial Pulsation must of Necessity go on, to carry Blood to every Part of the Body, being propell'd by the same constant Force always, in healthy Bodies. Hence the Convenience of this our System of muscular Motion is apparent, since it is carried on at the same time that the Heart and Arteries do their Offices without Interruption to either.

XXVI.

Again, if it was absolutely necessary to muscular Motion, that Blood should be taken from an Antagonist, in order to be sent to the acting Muscle to break the *Equilibrium*, some Muscles, that serve

to involuntary Motion, could not have any Motion at all, having no Antagonist. Therefore, in general, Motion must be caried on more simply, and in the Manner explain'd above.

XXVII.

Yet an *Equilibrium* between a Muscle and its Antagonist is absolutely necessary; but it is only so in order to preserve the Equipoise between them, while both are in a State of Rest. Now there are two Sorts of *Equilibriums* proper to Muscles, the one which is peculiar to each Muscle in itself, explained before, and the other that which is between two Antagonists. Let us see how the latter is broke, in order to Motion.

XXVIII.

We are to consider two Antagonists exactly equilibrated, before any Action is begun in either. The Mind, now resolving to act with one Muscle, makes an Impulse upon the Nerve leading to it. The *Cells* in that Muscle are instantly inflated beyond their Tone of Rest, to the Degree necessary for the particular Action intended: The Blood is stopp'd, as mentioned before, in the little *venal parallel Canals*; and the Muscle is contracted beyond its Tone of Rest. And at the same Instant that the Mind impels the *inflating Matter* into the *Cells* of the Muscle in Motion, she remits to determine any to the Antagonist, which causes a *Retraction* of it into the Nerves: By this means, the interstitial Air, in this, exerts its elastic Force, compresses the Sides of the *Cells*,
whereby

whereby they are reduced to oblong Forms, and the whole Muscle is drawn beyond its Tone of Rest.

XXIX.

It is thus alone the *Equilibrium* is altered between two Antagonists in an Instant, without a Necessity of taking one Drop of Blood from the one to add to the other: And it is even so far from it, that, during all this Action, the Quantity of Blood in both Muscles, while one is contracted, and the other dilated, is nearly equal; for altho' the Blood is stopped in the acting Muscle, by the Mechanism above explain'd, and the Veins are all compressed, so as to contain none during a violent Contraction; yet the Arteries may be said to contain a double Quantity, propell'd into them by the constant Pulse, as they are elastic, while the Circulation is carried on pretty equally in the Antagonist; and consequently both Arteries and Veins in this can contain no more than the very Arteries in that: Therefore a Muscle is neither larger nor less, in general, by Contraction or Extension, than when in a State of Rest, the Proportion being constantly kept up; that is, when shorter, a little thicker; when longer, a little more slender.

XXX.

Our Explanation of the *Equilibrium* between the interstitial Air, and the muscular Cells, will enable us to attempt accounting for the swiftest gentle muscular Motions that can be perform'd; such as are necessary in an *Allegro* Part on an Instrument of Music.

Muscle. Thus: The Mind intends moving a Finger to perform a swift Shake; the first Motion is a Pressure of the Finger upon the String, by the Contraction of the *Flexor* of that Finger: The successive Motion then will be only a Remission of that Pressure, without any Necessity for a Re-action in the antagonist Muscle, because the Motion required is but inconsiderable: And this Pressure and Remission, by being nimbly repeated, will become a kind of alternate *Tremor* of the Finger upon the String; because, after the first Inflation of the muscular Cells, which causes the Contraction of the Finger, the instant Remission of the Inflation becomes a kind of Retraction of it, as I have said before: Then the interstitial Air follows it with an elastic Expansion beyond its Tone of Rest, and compresses the Cells; then a new Inflation of the Cells succeeds; and, in fine, an Action and Re-action between the interstitial Air and Cells is carried on as long as the Shake is required to continue: This amounts to an alternate *Systole* and *Diastole* between them, like two Springs acting by alternate Repulsion.

XXXI.

Of involuntary Motion.

The Performance of the common Actions of Life depending upon the voluntary Motion of the Muscles alone, Authors have almost neglected to give any Explanation of involuntary Motion; and therefore have chiefly attended to the former: Whereas the latter as well deserves the Attention of the Learned; and

and depends as much upon the same mechanical Principles and Structure, differing only in this, that voluntary Motion is urged by the Will, and the other is carried on without her Impulse.

XXXII.

It appears, by what we have already said, that the Disposition of muscular Fibres to contract depends on an additional Inflation of their Cells, but that this cannot be done without an impelling Power upon the Nerve to cause it. Now, in the Performance of voluntary Motion, the Will must be supposed to make this Impulse; but we must endeavour to find some other impelling Cause of Motion in the Muscles of involuntary Motion to set them to Work: For the mechanical Structure of all Muscles is alike.

We will produce two Examples; the *Heart*, and *intestinal Canal*.

XXXIII.

By a very natural Analogy between Vegetables and Animals, I am apt to believe, the Parts of the Animal are perfectly formed in the *Ovum*, before its Egress from the *Ovarium*, only wanting *Explication* and *Expansion*; but that these two Effects cannot be produced, till after it has changed its Place; and is deposited in such a Receptacle as may favour the Propagation of these Effects. Just so it is by the Seeds of Vegetables.

This being the Case, in order to come at our Purpose concerning involuntary Motion, we must now consider the *Fœtus* in the *Uterus*, tho' very small,

small, as having all its Parts complete, but motionless, or in a State of Rest.

XXXIV.

Now the Heart is the first Part that can have any Motion at all; and, being in a State of Rest, must be supposed in a *Medium* between its Dilatation and Contraction. The first Motion in the Heart then will be a compulsory Motion, which is its *Diaſtole*; and the first Motion performed by itself will be its reſtitutive Motion or *Syſtole*. Till this is done, there probably can be no Accretion of Parts, because nothing can be propell'd to the Extremities but by the Contraction of the Heart. And this is much earlier brought about than many Authors have imagined, and in the Manner following.

XXXV.

It is certain, that Fluids paſs from the Mother to the Child very early, by the Veins of the *Funis umbilicalis*, and at length arrive at the *Vena cava*.

The Auricles and Ventricles are now in a State of Rest; but the Fluids falling into the right Auricle, with a Gravity and Force it was not liable to before, it is fill'd and dilated beyond its former Tone; whereby the muscular Cells are laterally compressed, and become longer by the diametrical Pressure, and the interstitial Air is also compressed into narrower Limits than before, and the whole Auricle is upon the Stretch: Thus its *Diaſtole* is compulsory.

XXXVI.

XXXVI.

Now this sudden Repletion, at the same time that it dilates the Auricle, makes such an Impulse upon its nervous System, that an Inflation succeeds in the Cells, whereby they increase in Diameter (the interstitial Air now concurring by its *Nisus* to restore itself); and these two Powers together, which, in performing voluntary Motions, are Antagonists to each other, are now forced to become joint Antagonists to the dilating Force of the intruding Fluid from the *Vena cava*, whereby the Auricle is contracted, and the Blood driven into the neighbouring Ventricle. Thus is the Motion propagated from Auricle to Ventricle, and from the Ventricles to the Arteries thro' the whole Body, which is carried on during Life.

XXXVII.

And as to the Intestines, as there can be no peristaltic Motion, till Deglutition is first performed, it is carried on by the same Mechanism: For, when the first Food is swallowed, being driven into the *Oesophagus*, it forces it open, and dilates the circular muscular Fibres beyond their Tone of Rest, compressing both the interstitial Air, and the Cells of the Fibres; which, restoring themselves gradually, again contract themselves successively downwards, till the Food is convey'd to the Stomach, and thence to the Intestines: And this is also carried on till Death, when once begun. I shall only trouble you with one short Section more, which is:

XXXVIII.

Of deprav'd Motions.

Palsies, Cramps, and Epilepsies, may be accounted for by this System.

May not the muscular Cells become rigid and hard, and not be capable of being dilated to the Degree necessary to carry on the Action and Reaction between them and the interstitial Air, in an equal manner; and therefore produce such irregular Attempts to Motion, as would amount to a paralytic *Tremor*? Frequent Drinking spirituous Liquors will render the Cells of the muscular Fibres rigid, and cause the same Effects; and in Fevers, with what is called a *Subsultus Tendinum*, a common Symptom arises, which is an imperfect Attempt to pull the Bed-cloaths, attended with a *Tremor*. and proceeds from the same Cause; for the violent Heat of the *Fever* has render'd all the Fibres and their Cells rigid, and, therefore, incapable of due Inflation; for that Motion is not made in the Tendon which we feel, but is produced by the irregular Twitchings of the muscular Fibres of that Tendon.

XXXIX.

Or may not these Cells be, too lax and weak, and so lose much of their Elasticity, and yield to the Entrance of too great a Quantity of the *inflating Matter* of the Nerves, whereby the interstitial Air may be constantly compressed, and the peculiar *Equilibrium* deprav'd, so as to produce Spasms,

Spasms, if partial; if total, an Epilepsy? Painful Cramps are occasioned by the Inflation of some of the muscular Fibres, while others are not affected in the same Muscle; and an unequal *Drag* or Contraction is painful, when a total one is more tolerable.

XL.

And as to the Decay of a Limb, it does not seem to me to happen from the Want of a nervous Juice, by way of Nourishment, as has been the common Opinion; but because, whether they be Muscles of voluntary or involuntary Motion, the *inflating Matter* not being able to reach the *Cells*, from an Obstruction in the Roots or any other Parts of the Nerves, so as to fill them up to an *Equilibrium* with the *interstitial Air*, this Air will expand itself beyond its *Medium*, and compress the little Twigs of the *parallel arterial Canals*, that go off to nourish the *Cells* of the muscular Fibres, and lie constantly upon them, and so stop their Nutrition from the Blood; while the *parallel Canals* themselves, being too considerable to be affected by the Pressure of the interstitial Air, continue on their Stream: Thus a Decay of the Limb, and Loss of Motion, is effected, while there is a Circulation continued thro' the Body of every Muscle in the Limb.

XLI.

The Hearts of certain Animals, taken out of the Body, will continue to move a considerable Time, because the Action and Reaction cannot cease at

once between the *muscular Cells* and the interstitial Air, no more than a pendulous Body can cease at once, after having been swung beyond its Point of Rest. The Reason is, that as long as the Heart remains warm, the elastic Force of the interstitial Air, and that of the inflated *Cells*, will have the same alternate Effect upon each other, till the external Cold, and Want of the gradual Supply of the inflating Matter, cause the Action and Re-action to become unequal, and so by degrees fix both: But, when the Motion ceases, it may be again excited for a little time by Pricking. This seems to make a new Impulse on the Matter yet remaining in the muscular Cells, which, acting by their Expansion, will compress the interstitial Air, and receive a Repulse from it for a few times faintly: But this Experiment will not answer, when the Heart is quite cold; but then, if warm Water be pour'd on it, there will be Motion again excited. This happens, because the interstitial Air, before condensed by the Cold, is now instantly rarefied and expanded, and therefore presses the *muscular Cells*; which also being warmed, the remaining inflating Matter will exert itself in its turn, and so continue an alternate Motion for a few times, till the Heart grows cold again. But this Experiment will not often answer; because, altho' the same interstitial Air remains, it is a Doubt whether the Matter of the *Cells* does not exhaust by degrees; in which Case there can be no more Re-action. I have two or three times observ'd, when the Skin of an Ox has been taken off, the muscular Fibres continued to move in Spasms, and ceased at Intervals; then re-assumed their Motion, going on thus till the whole Surface

Surface grew cold, and the inflating Matter of the Cells, for want of a Supply, was quite overcome by the Pressure of its Antagonist, the interstitial Air.

XLII.

There might be many more Observations made upon this Plan, with regard to the Diseases *commonly* called *nervous* ; but as these Lectures were confined to *Physiology* alone, we shall make no Excursion from the Design of the Founder ; and shall be extremely happy, if this most learned *Society* shall approve of this Essay I have now made, in Obedience to their Commands.

The End of the Second Lecture.

LECTURE III.

SECT. I.

Read in October, 1745. **I**N performing the Lectures of last Year, besides laying open the Opinions of the most remarkable Authors who have endeavour'd to account for muscular Motion, I have given my own Essay towards its Explanation, which will appear to differ from every Opinion before it. And as I have there produced every thing I could say upon the Subject, touching the *Cause* and *Manner* of a Muscle's being moved, I must confine this Lecture to the Description of certain particular Organs, which, tho' not commonly thought so, I shall endeavour to prove to be Muscles: And these are, 1. The *Uterus* itself; 2. The *Fallopian Tubes*; and, 3. The Parts commonly called *Ligamenta rotunda*.

II.

Of the Uterus.

Whoever considers the Offices and Use of the *Uterus*, will hardly hesitate to pronounce it a *Muscle*

cle with *me*. Besides which, the very Structure of its Substance, from many Observations, prove it sufficiently, having discerned its interlaced muscular Fibres, as plainly as those of any other Muscle in the Body: However, its Definition and Use, as we shall now mention them, will serve further to illustrate and confirm our Assertion.

III.

The *Uterus* is an Organ destined, by the Wise AUTHOR of Nature, not only to receive, cherish, and increase the *Ovum*, which contains the *Fœtus*, till it arrives at due Maturity, but also to promote its *Expulsion*, when the Time of Gestation is fulfilled.

IV.

In a *virgin* or *empty* State, it is very small, as it appears at TAB. II. *Fig. 2.*; and being laid open, its Substance and Cavity are no more considerable than *Fig. 3.* represents; yet, in proportion to its Size, its *Parietes* are thicker and more compact, than at any time during its Gestation, whatsoever may vulgarly be thought of it by those who have only read of, and not observed it. This Substance, then, consists of fleshy Fibres and Blood vessels, both Arteries and Veins; and is cloathed internally by a Membrane, whose whole Surface is set thick with *valvulous Holes*, and externally by the *Peritonæum*: These, no doubt, serve to strengthen its Substance, and hinder its muscular Fibres from being driven too far asunder, by the Distension or Enlargement of the Blood-vessels (which

(which are very great and numerous thro' its whole Substance), while it is distending gradually, by the Increase of the lymphatic Fluid *, and Growth of the *Fœtus*, *Placenta*, &c. in it.

V.

In this virgin or empty State, the muscular Part is so squeezed and compressed together, that the Arteries and Veins are quite closed up, and utterly incapable of receiving a Drop of Blood during that State; except some few small Vessels, which only serve for the Continuation of Nutrition: But as soon as the *Ovum* is lodged in the *Uterus*, and begins to increase in Bulk by the Intrusion of the *Liquor Amnii*, it forces the *Uterus* to distend by degrees, till it arrives at the Bulk we see in the latter Months of Pregnancy. By this gradual Distension, the muscular Fibres are forced further asunder, and consequently the Blood-vessels are gradually freed from the Pressure, till at length they increase to their full Dimensions.

VI.

All this time the muscular Part, being compell'd to dilate, can perform no Function proper to a Muscle, until the *Cervix Uteri* is so shortened, as to become thinner than the rest of the *Uterus*; which does not happen till about the Completion of the
ninth

* *Liquor Amnii*. The Manner of the Secretion of this Fluid, which is much controverted, shall be soon accounted for in a Treatise on the Nutrition of the *Fœtus*, which shall be published in ~~the~~ *Essays on the Diseases of Women*.

ninth Month (a little sooner, or a little later). Now a Word or two of the *Cervix* here, since we are obliged to mention it, will render our Explanation more clear, and will also shew the infinite Wisdom of the *Great CREATOR* in the Formation of this Organ, for the Purposes it is design'd for.

VII.

The *Cervix* of the *Uterus* is a compact Part, narrower than the Body, and about an Inch long (more or less), as at TAB. II. *Fig. 3.* It has two Orifices proper to it while in this State; the one called the *internal*, which opens into the Cavity of the *Uterus*; and the external Orifice, commonly called the *Os Tincæ*, which opens into the *Vagina*. Its Substance is very hard and solid, and it has a very small Passage from the one Orifice to the other, having on its Surface many Glands, which secrete a dense glutinous Substance, and many *Striæ*, to which this *Gluten* adheres; so careful is Nature to keep the *Uterus* close, in order to prevent the Ingress of even the Air.

VIII.

Now this *Cervix* is made thus long and compact, that its Resistance to the intruding Waters of the *Amnium** should be greater than that of the *Parietes* of the *Uterus*, while it is dilating: This gives Time enough and Room for the *Fœtus* to grow to Perfection; and near the Time in which this is brought about, then this, which was before a *Cervix* with an Orifice at each End, is now become so much

L

shortened,

* *Ἀμνίον*, v. τὸ. The Name of one of the Membranes that contain the Child, &c.

shortened, as to acquire a greater Thinness than the *Parietes* of the *Uterus*, and consequently to bring both Orifices into one.

IX.

By this time the *Fœtus* being come to Perfection, and the Resistance being less at this Orifice than at the other Parts: The *Uterus* now begins to act in its muscular Capacity, beginning by degrees to contract itself alternately, till the Waters and *Fœtus* are determined downwards to the Orifice; which, the more it dilates, the more Power the *Uterus* still acquires, by its natural Disposition to contract; until the Muscles of the *Abdomen* are at length drawn into Consent, and the *Conatus*, or Pains, as Women call them, which begin by the involuntary Motion of the *Uterus*, are assisted by the voluntary Contraction of the abdominal and other Muscles towards the Extrusion of what it contains *.

X.

Nor does the *Uterus* rest, after it is rid of every thing, but still continues alternately to contract; until, in some Days sooner or later, it has squeezed out the Fluids from the Blood-vessels, called *Lochia*, compressed them close, and at length acquir'd its former Size and Compactness, or very near it. This is
what

* This regards the natural Delivery of a Woman in due time. As to what relates to Abortions at different Times, it is a Subject reserved for another Place.

what causes those Pains, which are commonly called After-Pains.

XI.

Thus is this wonderful *Phænomenon* brought about by the muscular Structure and Mechanism of the *Uterus*; and it may securely be affirmed, that if it was not a very Muscle, this Effect could not come to pass, no more than the Blood could be driven from the Ventricles of the *Heart*, if it were not a *Muscle*, or the Urine from the Bladder (without a muscular Coat, and a *Detrusor* Muscle), which are forced to distend by the Intrusion of Urine from the Ureters, till, by its Repletion, the Resistance becomes less at the Orifice than in the *Detrusor*, This is more fully explained in my Description of the Bladder; which see.

XII.

Of the Fallopian Tubes.

These Organs were known to, and well described by, *Hierophilus* and *Ruffus Ephesus*, as the late learned Dr. *Douglafs* has very justly observed; altho' they are called by *Fallopious's* Name by some not well versed in the History of Anatomy. They are soft pliable Bodies, and are properly enough called Tubes or Trumpets, because they arise small on each Side from the Angles of the *Uterus*, and run larger by degrees, till they

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they approach the Extremity, and growing again a little more narrow, terminate in ragged *Fimbriae*.

XIII.

The Cavity of each is also so small at their Origin in the Angles of the Womb, that a small Hog's Bristle can hardly be introduced; but it enlarges gradually, till, at the other Extremity, it is capable of admitting the End of a Goose-Quill.

XIV.

These Tubes are in a strait Direction for a little Way, from the *Uterus*; but as they enlarge, they grow into vermicular Curves or Flexures, terminating downwards, and a little backward, with their Mouths opening upon the *Ovaria*, altho' not at all attached to them; and are sustained and strengthened by their Situation in the Duplicatures of the *Peritonæum* on each Side, which serves to keep them in their curved State (as the Mesentery sustains the Guts) during the unimpregnated State of the *Uterus*: And these Duplicatures (which also strengthen, and in some measure cloathē, the *Ligamenta rotunda*, and which we shall speak to presently) are what are commonly called the *Ala Vespertilionis*.

XV.

These Tubes are made up of muscular Fibres, which are partly longitudinal, and partly are situated obliquely, and somewhat circular; from which Structure they have a Motion which may be called a compound Motion, and which amounts to what we count

count the vermicular Motion in the Guts; but this Motion is not begun, till there is a Necessity for it; which is as soon as an *Ovum* is impregnated.

XVI.

At the Instant that this Impregnation happens, the Orifice of the *Fallopian* Tube clasps itself close upon the Part of the *Ovarium* next to it, by contracting; and receives into it the *Ovum*; which, as soon as it has enter'd, those oblique circular Fibres, or, as we may term the Extremity, the *Sphincter* of the Tube, incloses and pushes it farther in towards the *Uterus*, which is by degrees forwarded by the peristaltic Motion of the Tube, till at length it is push'd into its Receptacle the *Uterus*, in the Manner explained in my last Lecture, where I endeavoured to account for involuntary Motion: And when the *Ovum* is thus settled, then the Cause of this Motion ceasing, the Tube is at Rest; and in proportion as the *Uterus* grows more tumid afterwards, both Tubes lose their vermicular Flexures, and at last grow quite strait and pendulous, as at TAB. III. *bb*, by the general Distension of the *Peritonæum*: But when the *Uterus* is emptied, and again is contracted to its former Dimensions, then the Duplication of the *Peritonæum* is contracted in proportion, and these Tubes are consequently restored to their former vermicular Flexures, and therefore rendered capable of receiving an impregnated *Ovum*, as before.

XVII.

From hence it is easy to conclude, that what has been thought, by many old Authors, and indeed by
some

some Moderns, concerning Superfetation (any considerable Time after the *Uterus* has received one (or more) impregnated *Ovum*) is altogether groundless: For, when the *Tubes* are grown flaccid, after having conveyed the *Ovum* to its Receptacle, and lost their vermicular Flexures, they can no more embrace the *Ovaria* during that Pregnancy; and also because the *Ovarium* on each Side is driven by the Distension of the *Uterus* to a greater Distance (out of its former Situation) from the *Vagina*, and consequently out of the Reach of Impregnation.

XVIII.

Therefore, whensoever it happens that two *Fœtus's* are brought forth at different Times, they both come within the common stated Time of Gestation; that is within nine Months, or thereabout, from the *Coit* that produced the Fecundation: In which Case, tho' one may be perfect, and come at the full Time, the other is imperfect, and sometimes wasted, and comes before the due Time, being both begotten at the same Instant, or within a very little Time of one another. But because one shall have more Nutrition determined to it, the other less, the latter, which is always situated nearest the Orifice, will of Necessity suffer Abortion. Thus, different Emissions of Children happen, not because they are begotten at different Times, but because, Nutrition being unequally distributed, the defrauded *Fœtus* is extruded by the other, who often keeps his Place till he arrives at due Perfection.

XIX.

Before we quit this Part of our Subject, it will not be unseasonable to mention a Case here, which was in some measure, objected to what I had laid down against a Possibility of Superfetation, when I read this Lecture.

A Gentlewoman in *Charles-Town* in *South Carolina*, about the Year 1714, was brought to Bed of Twins, one immediately after the other, in the same Labour. The one proved to be a *Negro* Child, and the other a white one; which very much surprised those that attended about her. So flagrant a Testimony of her Infidelity to her Husband, made her confess before them, that a *Negro* Servant, immediately after her Husband had left her one Morning, came to her; but she pleaded, as an Excuse, that he threatened to kill her, if she did not comply with his Desire; and that accordingly she was forced to admit him into Bed.

XX.

Now; In order to remove this Objection, it must be observed, that what I have asserted is, that when the *Fallopian* Tubes are grown flaccid, and have lost their vermicular Flexures, and the *Ovaria* are driven to a greater Distance from the *Vagina* by the Enlargement of the *Uterus*, that then they are out of the Reach of Impregnation.

Whereas, in the Case before us, there could be no such Change brought about in the *Uterus*; for, altho' one Tube had received an impregnated *Ovum* by the *Cort* of the Husband, and convey'd it to the
Uterus,

Uterus, whereby it may have lost its Flexures, yet the other Tube had not; nor could the *Uterus* be enlarged by the one in so short a time; for it appears the *Negro* cohabited with her immediately after her Husband; so that the *Ovum* impregnated by him was from the other *Ovarium* thro' its neighbouring *Tube*; which might have happened in a second *Coit* with her Husband, as well as with the *Negro*.

From this Structure and Office in these *Tubes*, we may venture to be of Opinion, that they are truly *Muscles*.

XXI.

Of the Ligamenta rotunda.

There have been various Conjectures concerning these slender Bodies amongst Authors; but the greater Part accounted them as Ligaments.

Spigelius, and after him *Diemerbroeck*, had a Notion, that they were *Vasa deferentia*, which they thought carried seminal Matter from the *Ovarium* to the *Clitoris* in Females! But this cannot be the Case; first, because they have no Cavity, that I could find, and are therefore impervious to any Matter: Again, their Situation would not admit of it, supposing they were tubular, because they arise from the Angles of the *Uterus* a little below, and forward of the Ligaments that suspend the *Ovaria*, as at TAB. II. *Fig. 1. (f)*; and, passing along thro' the Duplicature of the *Peritonaeum* on each Side, rise over the Edges of the *Os Pubis* nearly in a Line above the Fore-Parts of the *Acetabula*; and then running

running thro' the Rings in the *Peritonæum*, and Tendons of the oblique Muscles, &c. turn downwards and forwards, towards each other, on the *Ossa Pubis*, till they are lost in the Fat of the *Mons Veneris*, and sometimes in the Groins; and consequently could not serve to any such Use, having no Communication with either *Ovarium* or *Clitoris*.

XXII.

The celebrated *Winslow* thinks them vascular Cords, arising from the Communication of the spermatic and hypogastric Vessels; and says, they ought to be esteemed as a particular Continuation of the Spermatics; and that he suspects they furnish the Matter which is secreted from the *Lacune*. Now these Cords are made up of muscular Fibres alone; for, by the most strict Examination I could make, the Fibres appear to me fleshy, and have a longitudinal Direction from one End to the other; and as to their being vascular, it is impossible, from their Situation, they should be so, because they have not the least Communication with these Blood-vessels he mentions, and have only minute Twigs, as every other Muscle has, for their Nourishment: And as to the *Lacune*, which are Glands on the Surface of the *Vagina* in several Parts, they are remote enough from any Communication with the Fibres of these muscular Cords, where they are lost in the Fat of the *Groin*, and *Mons Veneris*; and therefore must be designed for some other Use.

XXIII.

From what I have said of these Cords, I must concur in the Opinion *Vesalius* had of them, concerning their being *Muscles*, who is quoted by *Sanctorius* on that account; and these, with our celebrated Anatomist Doctor *Nichols*, are the only Authors that I know of, that have mentioned them as Muscles; yet none, I believe, have ascribed the same Use to them that I think they are employed in; and that only respects the distended *Uterus*, as follows:

XXIV.

While the *Uterus* is in its contracted State, these muscular Cords are also in a State of Rest, forming the curved Direction we have mentioned, as at TAB. II. (f); and as the *Uterus* grows larger by its Pregnancy, they are pull'd upward and outward over the *Ossa Iliæ*, being more and more distended, in proportion with the *Uterus*, till they form nearly right Lines from their Origin to their Insertion, and are in many Women in the last Month 15 or 16 Inches long, more or less (See TAB. 3. d d.). Whereas, when they are in their State of Rest, they seldom exceed from 7 to 9 Inches. From hence it may be rationally concluded, that when Delivery is over, they assist the *Uterus*, by their equally contracting on each Side, the more regularly to contract itself, because many Evils might be produced by an unequal Contraction of it; but, as they are Part of its Discases, they can have no Place in this Lecture. And as the muscular Cords can in no wise be said to suspend the *Uterus*, we cannot consent at all to call them *Ligaments*;

ments; nor can we conceive any Use of them while the *Uterus* is unimpregnated at any time. Therefore we must think they are *Muscles*, reserved for the Purpose just mentioned; and neither *Vasa deferentia*, Blood-vessels of any kind, nor Ligaments.

XXV.

Thus I have finished the Task, which the worthy President and Council of this learned *Society* were pleased to charge me with: I return them my Thanks for the Honour they have done me; and make no Doubt but, whosoever shall be appointed to succeed me in these Lectures, will add considerably to whatever I have been able to advance upon these Subjects.

The End of the Third Lecture.

TABLE I.

Fig. 1. 1, 2, 3, 4, shew the Forms of several muscular Fibres, considerably magnify'd by the double Microscope, where they appear to differ in Size, as well as in the Number and Distance of their *Cells*, as it is mentioned in their Description before.

Fig. 2. is a View of several muscular Fibres, with those minute Blood-vessels which I have called the *Parallel or Capillary Canals* (see *Lect. II. Sect. XVI. and XVII.*) lying in their parallel Directions, in order to answer the Ends of Nature; which are explain'd in the Course of that Lecture.

Fig. 3. is a Representation of the Circulation of the Blood in a little Piece of the *Mytilus*, not exceeding a Quarter of an Inch every way in its natural Dimensions; wherein it appears, that those minute Vessels are in a Direction parallel to other Fibres of whatsoever Nature.

Fig. 4. shews Part of a *Capillary Canal*, with Part of a *Muscular Fibre* on each Side; to demonstrate how the minute Twigs are sent off from the Canal to the muscular Fibres to nourish them, as is hinted in the last Section above mentioned. This is view'd by the greatest Magnifier of the double reflecting Microscope.

TABLE II.

Fig. 1. is a View of the *Pelvis*, with the internal feminine Parts of Generation *in Situ*.

a, the Body of the *Uterus*.

b, the *Rectum*, turned over the upper *Vertebra* of the *Os sacrum*.

c, the Bladder.

d, the *Fallopian Tube*.

e, the *Ovarium*.

f, the slender muscular *Cord*, commonly called *Ligamentum rotundum*, rising from the Angle of the *Uterus* on each Side, and ending by several *Fimbriae* at *g* in the Fat of the *Mons Veneris*.

h, the *Meatus urinarius*.

i, the Orifice of the *Vagina* deprived of the Integuments.

Fig. 2. is a View of the Shape of an unimpregnated *Uterus*, all the Appendices being cut off.

a, the Body of the *Uterus*.

b, the *Cervix*.

c, Part of the *Vagina*.

Fig. 3. shews one Half of the same *Uterus*, the other being cut off laterally and longitudinally, whereby its Cavity (*a*), the inner Surface of its *Cervix* (*b*), one Lip of its *Os Tincæ* (*c*), and Part of the inner Surface of the *Vagina* (*d*), come into View.

e, the small Passage from the Angle of the *Uterus* into the *Fallopian Tubes*.

TABLE III.

Shews a View of a pregnant *Uterus* of seven Months.

a, the Body of the *Uterus*.

bb, the *Fallopian Tubes* relaxed and pendulous.

cc, the *Ovaria* and pendulous.

dd, the *Muscular Cord*, called *Ligamentum rotundum*, upon the Stretch.

e, the Bladder.

F, F, the *Ossa innominata*.

F I N I S.

